ON

THE PATHOLOGY

OF

DELIRIUM TREMENS;

AND ITS

TREATMENT WITHOUT STIMULANTS OR OPIATES.

BY

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PREFACE.

At the suggestion of several professional friends, I have been induced to reprint and publish the following paper, which lately appeared in the *Monthly Journal of Medical Science;* but, in order further to illustrate the opinions therein advanced, three additional cases of Delirium Tremens are now detailed: and I have introduced also the result of inquiries made at the Medical Officers of some of our Prison Establishments, in regard to their experience of the effects produced by the sudden withdrawal of wonted stimulants, in the case of civil and criminal prisoners, of known intemperate habits.

The only other additions of any importance which I have now made are references to the opinions given by Dr Craigie, in his very able Dissertation on Delirium Tremens (Methystic Brain Fever, Practice of Physic, vol. ii., p. 50, etc.); and I take this opportunity of expressing regret that I had not seen that paper previous to the publication of my own views, both on account of the similarity of opinions held on some points, and the very full and learned notice given by him of the literature of the disease. It will be seen, however, by any one acquainted with his observations, that we differ materially on many points connected with the phenomena, pathology, and treatment of the disease; and that what he has merely hinted at in regard to the affection, as being attributable partly to the cerebral vessels loaded with imperfectly aerated, spirit-charged, or alcoholized blood (pp. 77 and 81), I have more fully stated, and endeavoured to explain and illustrate, as a peculiar toxicological result.

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ON DELIRIUM TREMENS.

BEFORE attempting to explain the following views of the pathology and treatment of delirium tremens, I think it necessary to state what are considered to be the symptoms of the disease in its genuine and uncomplicated form; for it appears to me that the opinions generally received and acted on in regard to this affection are erroneous, and have resulted from a loose and partial observation of its phenomena, and inattention to the history of individual cases. In consequence of this, the disease is frequently confounded with very dissimilar affections, or its usual features lost sight of under the effects of injurious treatment, and its true nature, therefore, misunderstood.

The most characteristic symptoms of delirium tremens are general muscular tremors—more especially of the hands, and of the tongue when protruded—along with complete sleeplessness, and delirium of a muttering, sight-seeing, bustling, abrupt, anxious, apprehensive kind. The affected has not ability to follow out a train of thought, to explain fully an illusion or perverted sensation, or to perform any act correctly; because he may be one moment rational and the next incoherent; now conscious of his real condition and of surrounding realities, and then again suddenly excited by the most ridiculous fancies—principally of a spectral kind—such as strange visitors in the shape of human beings, devils, cats, rats, snakes, etc.; or by alarming occurrences, such as robberies, fires, pursuit for crimes, and the like. He is easily pleased and satisfied by gentleness and indulgence, and much fretted and agitated by restraint and opposition. The face is generally of a pale dirty colour, and wearing an anxious expression; eyes startled but lustreless, sometimes considerably suf-

fused, and the pupils not contracted unless considerable doses of opium have been administered, or very decided arachnitic symptoms have supervened; skin warm and moist, often perspiring copiously; tongue sometimes loaded but generally pale and moist, occasionally remarkably clean; appetite small, but the patient will often take whatever is presented to him; thirst by no means urgent, and seldom or never any craving for spirituous liquors; urine scanty and high coloured, and in some cases which I have tested, containing a large quantity of albumen, which, however, disappears immediately after the paroxysm is over;[1] alvine evacuations bilious and offensive; and the pulse usually ranges from 90 to 120, generally soft, but of various degrees of fulness and smallness, according to the strength of the patient and the stage of the affection. The precursory symptoms are by no means peculiar or pathognomonic, but common to many febrile affections implicating the sensorium in the way of sleepless and restless nights, with, perhaps, more of a hurried and agitated manner than usual for some days previously. The paroxysm, which is distinguished by the phenomena above described—occurring with remarkable uniformity, independently of age and constitution,[2]—usually runs its course, if uncomplicated and pro-

[1] Probably albumen is always present. In the thirtieth volume of the Transactions of the Medico-Chirurgical Society of London, Dr Bence Jones has endeavoured to prove, from three cases of this disease, compared with three of phrenitis, that the phosphates are found in much smaller proportion in the urine of the former than in the latter, and that thus a new diagnostic mark is presented of the nervous and non-inflammatory nature of delirium tremens. His observations, however, are not sufficiently numerous to be conclusive, and there are many probable sources of error. In the only two instances (Cases VII. and IX.), in which I have examined the urine, there were present in the one a great many, and in the other a considerable number of beautiful phosphatic stellæ; and I have no doubt that these will often be found in abundance in the most genuine cases of the affection.

[2] The age at which delirium tremens most commonly occurs, seems to be between 35 and 50; the constitution is the irritable, excitable, and somewhat weakly; and what is interesting, it is very uncommon in the female, although from what cause I cannot explain, for the number affected does not bear any relative proportion to the number of dram-drinkers. I have seen several cases of the delirium ebriosum, but no instance of delirium tremens in the female sex. Rayer observed the disease in 7 women out of 176 cases; Bang in 10 out of 456 cases; and Dr Hoegh-Guldberg of Copenhagen (*Commentatio de Delirio Tremente*), in 1 out of 173 cases. Dr Gibson of Glasgow, however, informs me that out of 57 cases treated in the prison of that city during 10 years, 8 were females.

perly treated, on the second or third day, though sometimes earlier, and it seldom extends beyond the fifth day. It then terminates in a profound natural sleep, which may continue for many hours, and from which, if it even lasts for six hours, the patient awakes quite coherent, although weak and languid, but from which state, considering the severity of the symptoms, he is restored with singular rapidity to physical strength and mental soundness. The casualties of the disease are convulsions or coma, which, if not immediately fatal, are apt to leave the unfortunate sufferer a wreck for the remainder of life.

The paroxysmal phenomena may occur variously modified in the progress of pneumonia, bronchitis, fever, erysipelas, and other diseases affecting the habitual drunkard, or after he has received a personal injury which occasions a severe shock to the system. I need scarcely add, that delirium tremens thus complicated, is frequently fatal under any mode of treatment; and the remarks about to be offered are not meant to apply to such instances. It may also be observed, that the foregoing account of the phenomena of the paroxysm is descriptive of the disease when running its ordinary course without being interfered with or obscured by the action of stimulants, opiates, or other treatment.

There is a form of *mania* which is sometimes mistaken for delirium tremens, but which must not be confounded with it, although characterised by very considerable muscular tremor. It is nothing more nor less than a severe and protracted form of intoxication,—an affection of the brain and membranes, in which there is great vascular excitement, resulting from the direct or immediate action of alcoholic liquors. Even so careful and discriminating a physician as Dr Watson, has noticed two cases[1] as instances of delirium tremens, the first of which partakes more, and the last entirely of the character of the affection which I am now about to describe. It has been styled by Darwin the *Delirium Ebriosum*. It originates from a single fit of intoxication, or at least from a short course of intemperance (in vulgar phrase, "a boose," or "a ramble") engaged in by persons of a peculiar mental constitution and temperament, and which is most commonly induced by some depressing emotion. It is marked by an uncontrollable desire for more drink, which, when gratified, excites to further imperious demands, begetting indecorous conduct, and engendering passions so wild and vicious, that when

[1] Principles and Practice of Physic, vol. i. pp. 394, 395.

the hereditary mental constitution is imperfect, and the previous moral habits loose or depraved, not unfrequently lead to the perpetration of violent and criminal acts. The other symptoms and circumstances characterising the paroxysm, are dry heat of skin, particularly of the scalp; sometimes considerable muscular tremor; flushed countenance; a sullen, determined, or fierce aspect; red, ferrety eyes—as in the cases of Dr Watson already noticed—; dry tongue; strong, quick pulse; and loss of appetite for everything but liquor—and that of the strongest kind—although in some instances, beastly ravenousness for anything or everything eatable which comes in the way, until the affection has attained its height, when loathing, sickness, and free vomiting take place, after which occurrence, recovery begins. This state may be brought on once in a lifetime, from some accidental circumstance leading to an act of intoxication; or it may be induced at particular periods—distant, perhaps, months or years—as in the case of those unfortunate individuals to whom the name of *dipsomaniacs*, or *oinomaniacs*, has been applied. The attack is in general easily overcome by the immediate withdrawal of all stimulants, confinement under the care of one or two firm minded and strong attendants, and the administration of emetics and purgatives. All who have witnessed the various forms of disease affecting the drunkard, will readily distinguish genuine cases of delirium tremens from this or other affections attended with delirium, as its character is so well marked.

The substance of the opinions generally held regarding the essential nature of delirium tremens may be stated to be—that it is a disease of exhaustion or irritation of nervous power, and that it has the habitual abuse of intoxicating liquors for its predisposing cause, and abstinence from, or the abstraction of, an accustomed stimulus for its exciting cause.

The first part of this definition, namely, that delirium tremens is a disease of exhaustion or irritation of nervous power, appears to me to be vague, and not easily explainable, either on the principles of physiology or pathology. Various authors,[1] in the most arbitrary manner, make use of the terms exhaustion and irritation, separately or together, as it seems to suit their purpose. One can understand what the term nervous irritation, or what that of nervous exhaustion

[1] Blake, On Delirium Tremens. Copland, Dict. Pract. Med., p. 497. Watson, Pract. of Physic, vol. i. pp. 400, 401. Carpenter, On the use and abuse of alcoholic liquors in health and disease, pp. 28, 29, 30; and others.

may mean, and also how the latter state may succeed to the former; but that these two conditions—so opposite in their nature, and standing more properly in the relation of cause and effect—should coexist in this disease, is not so easily comprehensible; and consequently I believe that the idea of exhaustion—as interpreted according to the common sense acceptation of the word, namely, weakness, has led to much error in the treatment of delirium tremens. The more that the history and phenomena of genuine cases of this malady are considered, the more numerous do the difficulties surrounding the above explanation become. It seems to be forgotten that the disease is not occasioned by a fit of drunkenness, but that it is the result of the long-continued excessive use of stimulants, and therefore on the ordinary and well understood physiological law that exhaustion succeeds excitement in an almost invariable ratio, this affection ought rather to follow the cessation of an out-and-out debauch, than a course of systematic imbibition. The affection is, I consider, quite specific and peculiar. It is something more than simple "nervous irritation" or "nervous irritability:" it is essentially a form of nervous poisoning, which, in every instance—whatever be the state of the constitution, or however combined or associated with other diseases—is distinguished by a very remarkable uniformity of phenomena. In every instance of delirium tremens, the stimulus or alcoholic principle, a powerful narcotico-acrid agent, in whatever way atomically combined or chemically changed after its introduction into the system, acts slowly on the nervous pulp through the medium of the circulation, poisons its substance and sets up at last what may be termed an alcoholic erythism, or, if I may be allowed the expression, an alcoholism.[1] This in turn, no doubt,

[1] Since writing the above, my attention has been drawn to the work of Dr Huss of Stockholm (see an able analysis in the Brit. and For. Med. Rev., No. XIII., 1851), on what he calls "Alcoholismus chronicus," or "the chronic alcohol disease." Delirium tremens, however, is not recognised under this appellation; and does not appear to be viewed as an alcoholism at all by this author. The term is applied solely to a group of affections of the nervous system occurring in those long addicted to the abuse of spirituous liquors. These are distinguished by tremors and jerkings of the voluntary muscles, and diminished or increased sensibilities of surface to a greater or less extent; they are of gradual development; and terminate very frequently in paralysis, epilepsy, or idiocy, without any notable pathological alterations of structure. Such symptoms and results, variously modified by and combined with organic disease, are by no means uncommon in this country, but they appear to be

produces a certain amount and kind of debility in the cerebral functions, but combined with over-action of the circulation through the membranes of the brain, constituting a decided form of irritation, the tendency of which, if not allayed by judicious treatment, is to inflammatory action, and serious encephalic mischief.[1] Scipio Pinel

much more frequent, and earlier developed in Sweden and the northern parts of Germany, owing, it is supposed, to the very pernicious composition of the alcoholic liquors in general use. "With but few exceptions the symptoms have been caused by the potato brandy, which is served out over the counter of the spirit shops to the lower classes of this metropolis. Spirit from grain is not common, and the distilled spirit freed from the volatile oil (finkelolja) does not suit the palled taste of the habitual spirit drinker. The presence or absence of this oil must be carefully borne in mind in estimating the causes of the disorder."—P. 34, *Dr Huss's work*. It further appears that these potatoes, skins and all, are generally diseased or decayed; that mildewed grain is also used, and various poisonous vegetable products, such as spurred rye, *lolium* (frequently mixed with bad barley), and the seeds of the *raphania raphanistrum*—which latter was by Linnæus himself thought to be the cause of this disease; and this poisonous mixture is likewise favoured by preparation in copper vessels.—*Review*, pp. 56, 57. While Dr Huss acknowledges that all this must greatly increase and confirm the maladies described by him, he regards the alcoholic principle as the chief cause. On the other hand, the reviewer expresses a doubt (p. 59) "whether the *alcoholismus chronicus* be really dependent even chiefly upon alcohol," and considers it probable "that its phenomena result from the habitual use of alcoholic drinks holding various (narcotic and acrid-narcotic) poisons in solution in different amounts, and differing in their nature and action, but all having in common, that they exercise a highly deleterious influence, especially on the *nervous* system." Besides the probability that the various maladies described by Dr Huss are not altogether owing to a condition of *alcoholism*, it must be borne in mind that there is a great want of uniformity in the character and course of the attending phenomena, that there is nothing in fact to point out a peculiar physiological action such as may be attributed solely to alcohol. It appears to me therefore that they must be regarded simply as bad effects from drinking habits on the cerebro-spinal system in particular, through the general impairment of the nutrition of the body; and be placed on the same footing as those diseases of the heart, arteries, liver, kidneys, and other organs with their respective functions, also resulting from intemperance, and with which the so-called alcoholismus chronicus is more or less associated. The application of the term *alcoholism* to such affections as these, appears to me to be inappropriate—that given by Romberg (*Dis. of Nervous System*), namely, *tremor potatorum*, is preferable; and in the following pages, I trust it will be apparent that if there is one disease more than another arising from habitual excess in alcoholic drinks, in which *a peculiar toxicological effect* is manifested, it is delirium tremens.

[1] Watson, Prac. of Phys., pp. 400, 401.

considered delirium tremens to be "a first degree of paralytic cerebritis;"[1] Dr Abercrombie appears to have considered it as "a dangerous form of meningitis;"[2] Dr Bright actually includes it among his cases of "arachnitis;"[3] Dr Hoegh-Guldberg[4] also views it as a febrile affection, indeed a species of arachnitis; and Frank, Speranza, Andreae, and several other writers, have entertained similar opinions.[5] Such opinions, I think, come nearer the truth in regard to the nature of the disease than those generally received; and they obtain strength from its symptoms, the injurious effect of stimulating treatment, and the appearances observed in fatal cases. The post-mortem cerebral changes in so far as regards paleness of tissue and shrunken convolutions, steatomatous and other alterations of the coats of the vessels, and to some extent also thickenings and opacity of the arachnoid, along with a large amount of subarachnoid and intraventricular serous effusion, are most likely to be found in those who have had several attacks of the disease, and who have long been noted dram-drinkers. But even after first attacks the membranes generally present great vascular fulness, the arachnoid is thickened, and under it, throughout the brain, and within the ventricles, there is very considerable serous effusion. Dr Craigie's opinion of the pathology of the affection—which he terms *Meningitis phantasmatophora*, or methystic brain fever (*μεθυω ebrius sum*)[6]—is the following:—

"In the early and incipient stage of methystic brain fever, the symptoms depend on irritation of the brain. That is to say, the meningeal veins are loaded with an unusual quantity of venous blood, and the arteries with imperfectly aerated arterial blood, both charged with spirituous particles; and as this circulates slowly, it irritates the brain, and disorders the cerebral functions, *first* of sensation and perception; *secondly*, of memory; *thirdly*, of fancy; and *fourthly*, of judgment. The cerebral irritation thus induced is the great cause of the sleeplessness and restlessness, as well as of the fantastic delirium and hallucinations. In this stage of the disease, which is the *erethismus cerebri abdominalis* of Töpken, the *delirium erethicum* of Hufeland, and the *encephalopathia* of Leveillé, the symptoms may subside spontaneously, or under the use of ap-

[1] Traité de Pathologie le Cérébrale, par Scipion Pinel, p. 400.

[2] Diseases of the Brain and Spinal Cord, p. 63.

[3] Medical Reports, vol. ii. p. 10.

[4] Commentatio de delirio tremente, quoted in Brit. and For. Med. Review, vol. vi. p. 328.

[5] Referred to by Dr Craigie, Pract. of Physic, vol. ii. p. 66, 67.

[6] Op. cit., p. 53.

propriate remedies, by the poisonous blood being eliminated in the manner of exeretions during sleep.

"If, however, the irritative action do not thus subside, if the vessels be not unloaded, and the circulation re-established, it is liable to become fixed in the form of inflammatory congestion, and to give rise to effusion of serum and other morbid products. This seems to be the *encephalitis* of Frank and Hildenbrand, and the Hirnentzündung of Andreae. Even without effusion of serum, the fatal termination may take place; but this result is much more frequent in consequence of effusion, sub-arachnoid, cerebral, and intraventricular. The disorder, therefore, though merely irritative in the early stage, from the unhealthy state of the blood sent to, and retained within the cerebral vessels, becomes at least in the latter stage congestive, and perhaps even inflammatory."[1]

To get rid of the difficulties which the above considerations raise up against the favourite theory regarding the essential nature of delirium tremens, some have spoken of it as occurring in two forms —asthenic or sthenic, congestive or inflammatory; but such distinctions, while presenting a fine hair-splitting diagnostic aspect, really serve a bad purpose, by originating perplexing doubts and difficulties. Like plumbism, mercurialism, ergotism, or narcotism, alcoholism is, manifestly, specific in its nature. Lead, mercury, and other agents, may affect individuals in different degrees from difference of age, constitution, continuance of exposure, etc.; or the effect—like that of the virus of small-pox—though the same in kind, may be modified in one case more than another. When alcoholic liquors have been long abused, the active principle appears to affect the system, by accumulation, like some other poisons. It has been supposed that the gastro-enteritic disorder, from long continued drinking, originates an attack of delirium tremens, through the medium of the solar or coeliac plexus, and affects the brain only in a secondary or sympathetic manner;[2] but although disorder of the stomach, liver, and other organs of disgestion is an ordinary result of drinking habits, yet thousands of individuals are affected in this way, and die in consequence, without ever suffering from delirium tremens. There is something more required to occasion this disease, the first manifestations of which are shown rather in disturbance of the organic functions by transmitted influence *from* the brain and nervous system to the digestive organs. The effect is brought about after the manner of a cumulative poison, the action of which is on the nervous centres. The experiments and observations of Dr

[1] Op. cit., pp. 81, 82.

[2] Göeden of Berlin, as quoted by Dr Craigie, op. cit., pp. 67, 68.

Percy[1] prove that alcohol has a peculiar favour for cerebral matter, fixing at once on it, by a sort of elective affinity; and indeed in fatal cases from direct intoxication, its actual presence in the substance of the brain is demonstrated. Now, in the longer continued abuse of alcoholic liquors, is it probable that the selection of the agent will be different? Its accumulation may be slow, and the change in chemical constitution may be considerable, but it is not the less sure. Every additional drop imbibed brings the grey matter of the brain into that state which assists in the development of the alcoholic erythism; and thereafter, while disturbing the functions of digestion, etc., occasions those relative changes in the sanguiferous system of the encephalon, the tendency of which is, as I have already affirmed, to pass from irritation, or abnormal activity of circulation and functions, to inflammatory action, according to the severity of the attack, and other circumstances. It therefore does appear strange that physicians while describing delirium tremens as a disease of exhausted or irritated nervous power caused by intemperance, should recommend and practise as a remedy the very agent which occasioned it, or another, namely, opium, which, although unable of itself to produce it—as I shall afterwards show,—greatly assists and hastens the affection in those who habitually indulge in intoxicating liquors; and moreover an agent, the physiological action of which is to occasion engorgement of the vessels of the brain—vessels already too highly charged with blood containing a poisonous ingredient. This is truly acting, to a certain extent, in the spirit of the homœopathic dogma—"*similia similibus curantur!*"[2] But of this more hereafter, for I am now brought to the consideration of the second position, which assumes in explanation of this affection, that while the abuse of intoxicating liquors is its predisposing, the abstraction of, or abstinence from an accustomed stimulus, is its exciting cause.

With the first part of this proposition I quite agree; but the opinion that the privation of a usual stimulus must be regarded as

[1] Experimental inquiry concerning the presence of alcohol in the ventricles of the brain.—*Prize Thesis*, Edin. 1839.

[2] Hahnemann, however, and his disciples, do not recognize alcohol as a curative agent; and, strangely inconsistent with the dogmas of their creed, say, (*Organon*, § CCLXXXVI., p. 329), "it is only the most simple of all excitants, wine and alcohol, that have their heating and intoxicating action *diminished* by dilution"! Opium is homœopathically recommended in delirium tremens, not on account of its sleep-giving power, but because its effects are supposed to resemble the symptoms of that malady!

the exciting cause of the malady, I consider to be entirely erroneous. Analogy, certainly, will not bear out the theory. Mercurial fumes, or the oxides of mercury, when long inhaled or absorbed into the body, as in the case of gilders, quicksilver-miners, and others, in the course of time produce an attack of shaking paralysis—the *tremblement mercurial* of the French pathologists; but will it be averred that the workmen long exposed are more likely to be affected with tremors, if removed from this poisonous atmosphere and occupation, than if they continued at their work? The reverse is well known to be the fact, not only in the case of such artizans, but of those also who are beginning to suffer in a somewhat similar way from lead poisoning. In both affections, when the symptoms are recent, a cure can only be effected by removal from the injurious occupation; otherwise the symptoms deepen with hourly increasing rapidity, until tremors are succeeded by sleeplessness, delirium, and, ultimately, coma.

But, then, even granting it possible that the privation of a wonted stimulus may be the exciting cause of delirium tremens, is it a fact that it is so?

The supporters of the theory now under discussion, with the exception of Dr Blake,[1] do not make a positive assertion on this point. They speak of the disease as "commonly resulting from the abstraction of the accustomed stimuli after a habitual or continued indulgence in it, or after a protracted fit of ebriety;"[2] or as, "chiefly when sobriety has followed a protracted debauch."[3] Again, "This disease most frequently occurs in habitual drunkards, and especially when after repeated fits of intoxication, they suddenly lessen, or leave off their ordinary stimulus for a time."[4] "Delirium tremens occurs more frequently when the accustomed stimulus is withheld."[5] "The disorder frequently does not show itself until the accustomed stimulus has been withdrawn for a certain period."[6] "Very frequently, from some cause or other, this habitual stimulus has been taken away."[7] The stimulus "has been, in general, suddenly withdrawn before the disease distinctly shows itself."[8] From these quotations it is apparent that some instances of the disease are admitted

[1] Op. cit., p. 23. [2] Copland, Dict. Pract. Med., p. 498, sect. 9.
[3] Ibid. sect. 10. [4] Armstrong, Practical Illustrations of Fever, 1819, p. 493.
[5] Carpenter, sect. 27. [6] Taylor, Med. Jurisprudence, p. 613.
[7] Watson, Practice of Physic, vol. i. p. 398.
[8] Alison, Path. and Pract. of Med., p. 734.

to occur without *any* privation of accustomed stimulants; and Dr Watson honestly says, "Sometimes it comes on in men who are perpetually fuddled, even although they have not intermitted their usual allowance of drink." It happened thus in several of the cases which I shall give at length in the present communication; and I could mention many other instances, in which there was no diminution in the quantity of liquor consumed—and some even in which there was a decided increase—up to the moment of seizure.

Since, then, intoxicating liquors may be, or rather often are, the directly exciting, as well as the predisposing cause, of delirium tremens, the second position in the theory—stated with all the precision of a law, and which, as such, ought to be invariable—is untenable; and as the idea expressed in the first part of the theory, that, namely, of exhaustion of nervous power, has been formed on the supposed correctness of the second proposition, the whole structure must be abandoned as without foundation. To me it is apparent, that habitual excess in the use of stimulants is alike the exciting and the predisposing cause of delirium tremens; and that if a suspension or diminution of habitual supplies be at any time attended by symptoms of the disease, these are not to be regarded as resulting from change in the quantity consumed, but as occurring in spite of such change, and because the peculiar constitutional effect has already been induced, and the premonitory stage of the affection already begun. I feel persuaded, that every practitioner who has seen much of this disease must, on an impartial review and consideration of his cases, confirm this remark. For my own part, I can affirm, that in a very considerable number of instances the patients were drinking freely up to the period when the disease was developed, there being no interval, and no diminution of quantity; and where there really was some diminution from the amount of previous supplies, it was on account of the system having already been brought into the condition of alcoholism, and a less quantity now produced a greater or equal effect, compared with that of the larger quantity taken formerly. There are, in some instances, no doubt, an entire cessation from the use of stimulants, or very nearly so, at the time when the symptoms of delirium tremens are in the course of development, but this is because no more can be taken by such individuals:—they are already saturated, as it were, with the alcoholic poison. From overlooking these circumstances, I believe all the statements in regard to the supposed effects of diminution, suspension, or abstraction of an accustomed stimulus

have originated. The error is a popular one, and has arisen from imperfect inquiry into the history of individual cases, and incorrect observation regarding the circumstances connected with the supposed reduction or abstraction. When called to see a case of delirium tremens, on inquiry as to the habits of the patient, we are frequently informed by his friends, that for a long time large quantities of spirits, or wine, or malt, or of all of these—and perhaps, in addition, morphia or opium—had been systematically consumed, but that for some time—a few weeks perhaps—much less had been taken, and within the last few days little or none; and then the inference is drawn for us, that the unfortunate patient has actually brought on the attack by meritorious efforts to free himself from a habit of which he had begun to be ashamed. Now all this is very plausible, but not in accordance with the strict facts of the case, as the individual himself, if put on his word of honour, will probably confess. The statement ought to be, that he was formerly in the habit of consuming large quantities of his favourite stimulant, until he found that a much less dose began to affect the system; that then he reduced the amount still further, but experienced an equal, if not greater, constitutional effect therefrom; and thus, from day to day, reduction was forced on him by his own sensations of gastric irritation, nervous excitement, and muscular debility,—these feelings having been, in fact, neither more nor less than the premonitory symptoms of the attack of delirium tremens, and just what might have been expected if—as I have ventured to assert—the alcoholic principle is to be viewed as a cumulative poison.

The habitual and excessive use of intoxicating liquor, however, does not affect all individuals alike. Some drinkers are early cut off by diseases of the liver, heart, or other organs, to which they may have a hereditary or constitutional liability; others in fevers or inflammations, which they have no stamina to contend against; others by apoplexy or paralysis, from the direct effects of a debauch; and some from hereditary predisposition, or otherwise, are early doomed to spend the remaining years of a miserable existence in mania, idiocy, or in a general paralytic condition of the system.[1] Some few drinkers, again, by reason of extraordinary constitutional vigour, escape all these ills, and live on to old age; but the greater number who are not early removed from society by the diseases enumerated, suffer more

[1] This comprehends the group of affections described by Huss under the name of the Chronic Alcohol Disease, and which has already been referred to.

or less from the attacks of delirium tremens. There appears to be a certain peculiarity of constitution which predisposes the individual to this malady—provided drink has been systematically indulged in,—that, namely, in which there is a highly sanguine temperament, and a nervous, irritable disposition. And I consider also, that the readiness with which the disease occurs, and the mode in which it is developed, are well explained by other individual peculiarities and circumstances. While one is attacked suddenly, without any diminution of the quantity, or change in the kind of stimulant, another is more slowly and gradually affected, and it may be, after very considerable reduction in the supply of liquor. In these respects, however, the effects of the stimulant are simply analogous to those of various other potent medicinal agents. For example, some constitutions are easily affected with mercury, others with difficulty. Salivation may all at once be displayed in one individual, who has taken the drug for a long time in very full doses, while in another it is produced by very gradual degrees, although with an equal amount of the mineral. But in both instances, after the constitutional effect is once produced, it may be kept up and increased to an excessive and serious extent by very small quantities of the mercury; and in the latter circumstances it would be very absurd to aver, that salivation was owing entirely to the more recent and smaller doses of the drug; or, still more absurd, if the mercury was altogether withdrawn, to say that the increase or continuance of salivation depended upon the abstraction. In like manner, when the nervous tissue of the brain has become charged with the alcoholic poison beyond a certain point, the effect it produces is kept up, and even increased, notwithstanding very considerable reduction in the amount consumed; and we are thus enabled to explain why diminution is almost universally supposed to be the cause of the malady, when, in fact, the indisposition to take more is itself one of the precursory symptoms of alcoholism. In the delirium ebriosum, there is urgent desire for drink during the violent stage of the affection, until a paroxysm of sickness occurs, which induces exhaustion, and then sleep; but in the delirium tremens, there is seldom any desire for it, even at the beginning of the attack, and certainly none when the affection is developed. Illustrations of the tendency to accumulation might be drawn from the effect of other medicinal agents, each acting in its own peculiar way, and on particular organs or functions. This, however, appears

unnecessary, for I think it has been clearly shown, that the alcoholic principle—imbibed systematically—passing through the channel of the blood, in whatever way atomically combined or chemically changed, has its influence concentrated on the nervous pulp of the brain, accomplishes its work on the perceptive, sensory, and motor powers,—in one case quickly, in another, if not soon, at last suddenly; or, by disturbing the varied functions of the economy, it induces such a condition of the system, that a smaller quantity taken will ultimately produce a more intense and lasting effect.

These views of the subject, to my apprehension, help to explain why delirium tremens is so readily brought on in dram-drinkers, when subjected to external injuries, or when seized with any kind of inflammation or fever. The sudden shock to the system in the one case, and the altered balance of the circulation and disorder of nutrition in the other, brings, I conceive, the individual at once into the condition of susceptibility to this disease, which would not otherwise perhaps have been so early accomplished. The effect is somewhat similar in the case of those who possess the gouty diathesis, for an injury of a limb is extremely apt to precipitate an attack of gout, which, in the ordinary course of events, would probably not have taken place for a considerable period of time. There is, apparently, in the habitual drinker of a nervous temperament, a tendency to delirium tremens, as there is in the *bon vivant* of a certain temperament to gout, and as there is in the epileptical or hysterical subject, to epilepsy and hysteria, although in each instance there is great dissimilarity as regards condition, cause, and effect; and any sudden excitement, shock, or severe malady which powerfully affects and disturbs the vascular and nervous systems in individuals so predisposed, may greatly aid in bringing on a characteristic attack or paroxysm. It is in this irritable state of the habitual drunkard's constitution, although he may not be on the verge of delirium tremens, that alcohol, by its presence in the blood—in whatever way combined—and its interference with the nutrition of the brain and nervous system, will superinduce on the receipt of an injury—say a gunshot wound, a severe burn, or a fracture—a febrile attack, attended by delirium presenting somewhat of the appearance of that disease, but which in reality has more of a typhoid character. This affection has been named by Dupuytren "*Delirium nervosum s. traumaticum;*"[1] and although some writers have considered it iden-

[1] "Annuaire Médico-Chirurgical des Hôpitaux."

tical with delirium tremens, it only similates it, being a symptom of the sympathetic fever which occurs under the circumstances above noticed. But whether delirium of this character, with some degree of tremor, takes place under these circumstances, or whether an attack of pure delirium tremens immediately supervenes on the receipt of the local injury or disease from that state of the constitution, and the previous habits of the individual, already explained, it is quite erroneous to suppose, in either case, that the affection originates from the suspension of the stimulants to which he had been previously accustomed, however plausible the theory may be which thus accounts for its production.

The idea, that bad consequences result from a sudden abstraction of stimulants, having got possession of the minds of many able writers on this malady, all their views of its nature have been perverted, and they have misled the profession into a dangerous system of treatment. Dr Blake, for example, who has been much quoted as an authority on delirium tremens, says: "It is purely idiopathic, arising *invariably* from the same cause, namely, the sudden cessation from, or a material diminution of, intemperate habits;"[1] and he goes so far as to assert that at "almost any time he *could have* brought on an attack of delirium tremens in the habitual drinker, by simply taking him into hospital for three or four days, and keeping him on spoon diet."[2] He does not appear, however, to have tried this experiment. It is an assumption from a theory supposed to be true, and has appeared plausible from the fact already admitted and explained, that the disease sometimes occurs in those taken into hospital on account of sudden or severe shocks to the nervous system from injuries and other maladies, but who have had the alcoholic erythism strongly developed, and who are, in fact, already on the verge of an attack of delirium tremens. The gourmand would feel equally weak and miserable, and his general tone be for a time depressed by the abstraction of good living; but however strongly the gouty diathesis was in such a case, this deprivation of good things would not occasion an attack of gout, although an injury of any kind, nay, a scratch, might. So I hesitate not to say, that the dram-drinker, in whom the delirium tremens diathesis is not yet fully established, and who is not already under the precursory symptoms of the disease, could not be subjected to a paroxysm by such treatment. From the sudden change on his circulation, he would doubtless experience

[1] Op. cit., p. 23. [2] Op. cit., p. 18.

much mental disquietude and physical discomfort, and be made "shaky," according to common phraseology, for a time; but this would soon pass off, without the occurrence of the usual pathognomonic signs of delirium tremens, more especially without those spectral illusions or phantasms which are common to poisonings, with several other agents of the narcotico-acrid class.

The opinion of Dr Craigie on this point is so decided, that I cannot resist quoting it. He says :—

"Without positively denying that the disease may come on in this manner, I can only say that I never witnessed an instance of this mode of development; and, after perusing all the published cases extant, I cannot perceive that any of them, excepting the one recorded by Dr Armstrong, in the 9th vol. of the 'Edinburgh Medical and Surgical Journal' (p. 146), afford satisfactory evidence that the disease is induced in consequence of the sudden abstraction of the use of spirituous liquors; and even that case, I think, may be explained without having recourse to the supposition now mentioned. I have, on the contrary, never observed that the sudden and complete abstraction of these liquors aggravated the symptoms of the disease. I find further, that neither Berndt, Toepkin, Hufeland, Andreae, Göeden, Sieburgundi, nor any other foreign physician by whom the disease has been observed, admit that it is produced in this manner; and in all the cases recorded by them, the symptoms were developed after a continuance, more or less protracted, of stimulation by spirituous liquors." [1]

In order to obtain some additional evidence on this disputed point, I submitted some queries to Drs Simson and Gibson, the medical officers of the large prison establishments of this city and of Glasgow, and to Mr Page and Dr Scott, surgeons to the county gaols of Carlisle and Dumfries; and the following information has been kindly furnished by them, as to the effect of the sudden withdrawal of all stimulants from civil and criminal prisoners known or presumed to be of intemperate habits, and the immediate substitution of prison fare, which is well known not to be of the most generous description.

As regards the prison of Carlisle, it appears that, although the annual number of commitments during the last fifteen years has been about 600; and that, although three-fourths of these are considered to have been, in one way or another, the consequences of drunkenness, Mr Page states emphatically he has never yet seen any ill "result from the sudden abstraction of stimulants from

[1] Op. cit., 57.

habitual drunkards, who had been drinking to excess up to the time of being placed on prison fare." Mr Page had also, during nine years' experience in connection with the Carlisle County Pauper Lunatic Asylum, observed the same impunity with which all stimulants could be at once withdrawn. (*Letters, 9th and 21st June* 1854.)

Of the gaol of Dumfries, it is stated by Dr Scott (*Letters, 12th and 21st June* 1854) that, during the last fifteen years, the number of civil and criminal prisoners have amounted to 5539; that of this number he supposes about two-thirds were committed for crimes resulting from intemperate habits; that he believes a very large number to have been habitual drunkards; and that, although all of these, of course, were deprived of their usual libations, and at once put on prison allowance, only five cases of delirium tremens are found on the register of disease, and that all of these patients but one were admitted to the prison with the disease on them; and that in regard to that one, although entered as under delirium tremens on the day after admission, there is every probability for believing that *she* had had the disease on her when admitted, although not reported to be ill. Dr Scott also notices, as an important fact, that during the time the railways were being constructed in the county of Dumfries, a very large number of navvies were committed to prison, who had led a very dissipated life for many months, and although deprived of liquor from the moment of apprehension, not a single case of delirium tremens occurred.

Then, as regards the prison of Glasgow, in which the annual commitments amount to upwards of 4000, the experience of the year 1850 is adduced by Dr Gibson (*Letter of 16th June* 1854), as affording an approximation to the facts wished to be elicited. A calculation made in that year showed that, while 4122 were imprisoned, the number of assaults, with few exceptions, committed under the influence of liquor, and "the drunk and disorderly," amounted to 1519; and of this number only three cases of delirium tremens occurred—a very small proportion indeed, especially when it is considered that the debtors, who are almost all habitual drunkards, and drinking up to the moment of incarceration, are not included in this list. Many hundreds more, therefore, may be considered to have belonged to the drunken population of the gaol. The average of the last ten years, however, is greater (5·7), there having been fifty-seven cases altogether during that period, but, after

all, this is a very small proportion to the number of dissipated and drunken characters gathered together there, and at once broken off from intemperate habits. Dr Gibson, however, states that he does not altogether enter into my views as to the proximate cause of delirium tremens, although he admits that "it does not so frequently occur as the advocates of the theory, which attributes it to the total withdrawal of accustomed stimuli, such as Blake and others are inclined to suppose;" and he mentions, in proof of his objection, that he had never seen it occur in less than twenty-four or beyond seventy-two hours after apprehension, which necessarily put a stop to dram-drinking. As I have already explained, however, and as the cases given at the conclusion of this paper will show, there is always, whether the individual is drinking much or little, more or less of a premonitory stage present in this affection, distinguished by digestive derangement, nervous irritability, restlessness, and sleeplessness, before much tremor is displayed, or any illusions manifested; and it is easy to suppose that these might not be brought immediately under the notice of the medical officer of a large criminal establishment, such as the Glasgow prison. But even granting that no incipient symptoms of the disease were observed, and that this proportion of the habitual drunkards were not quite on the verge of being affected with it, it is quite in accordance with the views already advanced to suppose that, when there was a certain amount of alcoholization existing, the disease might be hurried on more speedily than otherwise would have been the case in individuals of a nervous and excitable temperament, by the agitation or shock of apprehension, and the deprivation of liberty. But, further, I should suppose it a very just, nay moderate calculation, to assume that out of a population of 2000 confirmed drunkards belonging to any class of society, although enjoying unrestrained liberty and uninterrupted opportunities for indulgence to excess, at least from three to six instances of delirium tremens would annually occur.

But, in fine, on this point, the evidence communicated by Dr Simson, the medical officer to the prison board of this city (*Letter, 4th July* 1854), is sufficiently satisfactory; for while the number of civil and criminal prisoners, committed during the last year, was 5864 (which may be assumed as a sample of the previous fourteen years, over which Dr Simson's experience extends), only four cases of delirium tremens occurred within the last eighteen months. The average number of cases during former years, Dr S. states as from

2 to 3 per annum. Dr S. considers that, at least one-half of the whole prisoners may be assumed as dissipated characters, and that at the very lowest computation, 500 must have been regular, systematic drunkards, from whom all drink was suddenly abstracted; and he goes on to state as his decided opinion, that "the sudden taking away of spirits, etc., does not produce delirium tremens. In *every* case, the prisoner had symptoms of the disease on him when admitted—that is, they were all restless, irritable, etc.; and I have, no doubt, but that in many instances the crimes committed were the effects of this disease. I do not remember a single case of delirium tremens occurring when the prisoner was quite well when received into prison. There is not the least doubt that a peculiarity of constitution predisposes to delirium tremens," etc.

Here then, it has been shown, that hundreds of individuals among the public at large, and of the criminals committed to our gaols, leave off or are suddenly deprived of the stimulants to which they had been previously addicted, without being seized with delirium tremens, or anything approaching to it.[1] On the other hand, also, it is unquestionable that numerous instances of the disease do occur in which there has been no suspension either voluntarily or by compulsion of the amount of liquor consumed, nay, even an increased excess in drinking up to the very moment of seizure.[2] The assumption, consequently, that this disease is produced invariably, or chiefly, or even occasionally, by the diminution or abstraction of an accustomed stimulus, is not supported by facts. Any cases, therefore, noticed as occurring under these circumstances, are simply of an exceptional character, but which in my apprehension, fall quite short of proof from the considerations already so fully explained.

Some even, borne away by the theory that delirium tremens is a disease simply of "exhausted nervous power," and refining on the idea, have gone so far as to aver that it may occur independently of the use of intoxicating liquors altogether. Thus it has been alleged to have been produced by the use and disuse of opium and of tobacco;

[1] Dr Scott has mentioned to me the case of a debtor who, although up to the hour of his incarceration in the Dumfries gaol, was in the practice of taking on an average, one bottle of spirits, and upwards of three ounces of laudanum daily, yet had every drop withdrawn without experiencing any bad symptom. This individual, too, it is interesting to know, had been twice previously the subject of delirium tremens.

[2] Cases II. III. VII. and X. are instances of this.

to have been resulted from protraeted mental applieation, from excessive depletions or evacuations, from rheumatism, from exposure to extreme eold, from hunger, ete., when no liquor of any kind had been indulged in. In all such instances there must have been some mistake or misapprehension. I ean suppose that the continued use of inordinate quantities of laudanum might occasion delirium tremens, as has been reported, from the amount of alcohol necessarily consumed, whieh of itself would be sufficient to occasion it, and more especially when combined with opium, which, as shall afterwards be shown, has a great influence in hastening its development; but that opium alone ever produced the disease I do not believe. I have never seen it; and when it has been supposed the cause, there must undoubtedly have existed some misapprehension of the history of the ease, or some concealment as to the previous habits of the individual, for nothing is more common than for an opium eater to indulge also in some stimulating beverage. Opium, when habitually taken by itself, may, in the course of time, break down mental and physical energies, giving the aspeet of premature old age, if not occasioning actual imbeeility or paralysis, but it will not produce delirium tremens; and when left off suddenly, the poor victim of the enslaving habit will for a time feel very wretched and feeble, but will not manifest the pathognomonic symptoms of delirium tremens, and will have the best chance of regaining to some extent his constitutional vigour. In regard to any influence whieh the disuse of tobaeeo may have in occasioning this malady, I would say that it is quite out of the question, and that any attaek occurring in the ease of the reeent smoker, must have been owing to the eonjoined habit of drinking. As an illustration of the erroneous notions prevailing in regard to the matter, and to the disease generally, I will give one of the last published instanees of delirium tremens—a good example of a mistaken cause, and a misunderstood effect—wrong theory, and wrong praetice. It is entitled "*Delirium Tremens produced by Abstinence from Tobacco.*"[1] The italics are introduced to draw attention to some points of importance.

"Delirium tremens, and its twin sister, traumatic delirium, are now so well understood to be dependent on *asthenic irritability of the nervous system*, that but one opinion prevails as to the principles which should regulate their treatment. *Sudden disuse of accustomed stimulants* is always to be deprecated, and

[1] Medical Times and Gazette, No. 163, Aug. 13, 1853.

in the event of a patient of known intemperate habits coming under surgical treatment, especially on account of an accident, care should always be taken that he is not deprived of his wonted allowance of alcohol. There is, however, another very potent drug in but too common use among the lower orders, the probable effects of suddenly relinquishing which, have, we suspect, been too little considered, and respecting which, the notes of a case lately under the care of Mr Curling, appear to offer a valuable hint to the practical surgeon. A withered old woman, *a gin drinker*, and a habitual smoker, was admitted on account of *a severe burn*. *Stimulants were from the first freely allowed her, and opiates administered*, but in spite of them she continued extremely restless, wandering at times, and quite unable to sleep. Her manner and aspect indeed *much resembled those of delirium tremens*. At this juncture, *several days after admission*, Mr C. ordered that she should be permitted to smoke. The salutary influence of the permission was at once apparent, the woman became quiet and tranquil, and on the next night slept fairly. All tendency to delirium disappeared, and she afterwards progressed steadily to recovery."

Now this was nothing but a mild case of delirium tremens, from habitual gin-drinking, precipitated by the severe burn, and aided by the stimulants and opiates so freely given; and in consequence of these combined circumstances—not in spite of them—the restlessness, etc., continued. The absence of the tobacco had nothing to do with this state of matters, but the disease originated from its ordinary cause, and was running its ordinary course of a few days. Convalescence was in all probability begun when the tobacco was allowed, but if not, no doubt its effect would be good, for it would act, not as a stimulant or a narcotic, but as a sedative—soothing and depressing the cerebral excitement—and sleep would follow as a natural consequence. It would not be surprising if tobacco alone, given in other cases, proved beneficial.

Then as regards the other causes, independently of alcoholic liquors, said to produce delirium tremens, the kind of delirium differs in each case, or partakes more of the characters of insanity; and there is also a corresponding diversity in the nature of the wakefulness, the muscular tremors and other symptoms, all of which circumstances, if space permitted, could be explained on very different grounds. But the mental phenomena of true delirium tremens, distinguished by a quick, eager, busy, apprehensive, spectral character, viewed in conjunction with the peculiar tremors, and sleeplessness, and with the uniform course and character of the other general symptoms, are surely sufficiently diagnostic. The term delirium tremens is, no doubt, pathologically incorrect, for incoherence and tremor may coexist in very dissimilar

states of mind and body, and originate from a diversity of causes; but it has been so long assumed by the profession, and known by the public as applicable to a disease originating solely from continued and excessive indulgence in alcoholic stimuli, that a more general signification cannot be recognised without leading to confusion and error. The cause and the course of this interesting disease are so very different from that of the affections above noticed, that no affinity in nature or pathology can be admitted. The functions of the brain in it are, I conceive, interfered with in consequence of the vitiated nutrition of its substance, and the irritation of the membranes. A peculiar erythism and excitement, as has been already asserted, is thus set up by the continued introduction and presence of the alcoholic poison; and every drop of intoxicating fluid now supplied to the circulation increases the poisonous action. If the supplies are still increased beyond this point—the furthest limit of endurance—the unfortunate individual will, in all probability, be seized with fatal convulsions or coma; or be cut off, or shattered for life, by the establishment of decided inflammatory action from protracted excitation of the brain and its membranes.

Now, if I have succeeded in showing that the alcoholic principle tends, by long-continued and excessive use, to occasion delirium tremens as the result of a specific poisonous action on cerebral matter; and that this happens on the principle of accumulation (as is the case with many other agents, such as mercury, lead, iodine, opium, Indian hemp, strychnia, etc., each acting in its own peculiar way), it should follow, that even a small quantity administered in the treatment of that disease, must necessarily increase the mischief instead of curing it. On theory, therefore, the rule of practice appears to be sufficiently evident; but whether or not the above reasonings and statements are considered sound and satisfactory, no inconsiderable amount of experience may be claimed in recommending an entirely non-stimulant and non-narcotic plan of treatment. I am aware that in advocating the disuse of stimulants and opiates I may be considered guilty of a medical heresy. The practice objected to has, there is reason to believe, been for long almost universally followed in this country, to a greater or less extent. It has arisen, partly, from blind adhesion to the popular error I have already attempted to expose, which assumes that delirium tremens originates from, and is aggravated by a diminution, suspension, or abstraction of an accustomed stimulus, and therefore to be treated successfully only

"by a hair of the dog that bit;"[1] and it has arisen partly also from acquiescence in those modern pathological notions which attribute so much to the disordered organisation, and diminished nervous power of the solids, and so little to vascular disturbance, to chemical change, and to poisonous action of the fluids of the body. It is pleasant to observe the spirit of inquiry now drifting towards the much neglected claims of a humoral pathology, and bent on investigating the nature and extent of blood-poisonings; and I despair not of seeing, ere long, still greater advances made in this direction.

As regards the treatment of delirium tremens on the views which I have endeavoured to unfold, the experience of upwards of fifteen years may be pleaded; and during the five previous years I also had ample opportunities of witnessing the practice of others, and of personally testing the merits of the mode of treatment ordinarily pursued. In the earlier period of practice the observations were made almost entirely in connection with hospital and dispensary attendance, affording a great many examples of the disease both in its pure and in its complicated forms, as occurring among tavern-keepers, brewers, butchers, and the lowest order of dram-drinkers generally; latterly the instances have been mostly among a better class of society, yet the disease presenting the same features, and originating from the same degrading cause. The frequent sudden fatalities which I have witnessed from arachnitis, convulsions, and coma, when stimulants and opiates were freely administered; and the length of time ere recovery took place, even in the most favourable instances of the malady, when these agents were given more sparingly and cautiously, long since convinced me that their tendency is highly dangerous. I do not say that I would never give a stimulant in delirium tremens. It may possibly happen, although I have never met with such a case, that in the advanced stage of the affection the pulse may begin to falter, the heart lose its usual rythm, the surface of the body to become of a leaden hue, the tremors to disappear, and subsultus tendinum occur, and delirium of a muttering character only continue, then certainly the flagging powers of life would require to be sustained by some diffusible stimulus. Here there would be no alternative. Then, again, I would not hesitate to give an allowance of his usual stimulant to a habitual drunkard

[1] The common practice has been, and, I have reason to know in many quarters still is, to give from one tablespoonful to a wine-glassful of spirits every two or three hours, either alone, or combined with opium.

when affected with a wound or ulcer, so as to obtain a healthy action therein; or to administer stimuli of one kind or another freely in ordinary fever, or in the typhoid state of traumatic delirium, so that his circulation may be enabled to keep up the functions of organic life until food be made use of. This would only be using legitimate means to maintain his ordinary condition of body; but it is quite another thing to prescribe alcohol when the individual is already manifestly in a state of alcoholic poisoning.

From all that I have seen and read, I believe that the combination of stimulants with opiates is a most hazardous practice in the treatment of delirium tremens; for while the former increases the determination of blood to the head, the latter is apt to occasion engorgement there, and thus, doubtless, they are the joint cause of many sudden deaths, and of many incurable palsies of body and mind—indeed of the great proportion of those casualties which take place, and for which the disease, and not the treatment, is blamed.

Opium given alone in delirium tremens is, I am aware, almost universally considered by the profession to be quite an indispensable agent—the *sine qua non*—for securing what is called the critical sleep; and hence it is prescribed in smaller or larger doses in as routine a manner as sulphur is for the itch, or colchicum for gout. Notwithstanding this high estimation of its value, however, I hesitate not to say that it is a very doubtful remedy even in the most promising cases of the disease, and a most dangerous one in others. It is well known that a moderate dose of opium in delirium tremens, so far as regards its action on the brain and nervous system, is in the first instance exciting and preventive of sleep. I have frequently seen such doses as in other affections would have been considered very large, in this greatly increase the agitation and excitement after each successive administration; and although sleep was secured at times, it was but short and disturbed, and followed by delirium as violent as before. Besides, the most unmanageable cases of delirium tremens which are met with, are those affecting opium or morphia eaters, who appear to be extremely liable to this disease if they indulge in spirituous liquors. From the use of opium or morphia alone, as already stated, true delirium tremens never occurs; but with the unfortunate slaves of this debasing habit, a very short course of intemperance is sufficient to develop it.[1] I have also remarked in

[1] See Case No. V.

several of these instances, that if, during the attack the usual dose of the narcotic is taken under the impression that it would soothe distress and procure sleep, more especially if that dose be morphia—which is apparently much more stimulating in this affection than opium—the paroxysm is greatly aggravated.[1] It is evident then, that if opium is to be used at all in delirium tremens, it must be given in a large dose (in from two to three or more grains, and repeated at intervals of a few hours); and it is thus generally given, the object being to overstep the stage of excitement, and force on the desired sleep. Now the acknowledged effect of a large opiate on the encephalon is to occasion engorgement of the vessels, more especially of the veins, and consequently, the larger the dose the greater will be the amount of sanguineous compression of the brain. What then must be the probable result in a disease in which there is already, if not an approach to arachnitis, at least a very excited action of the meninges, and a preternatural loading of the vessels generally? The cerebral functions are oppressed, and at length overwhelmed, and sub-arachnoid effusion is the result. The symptoms attending this untoward event are characteristic. Sleep is obtained, but it goes on deepening, and, as it becomes more profound, the pulse becomes smaller and less frequent, the surface of the body covered with a cold sweat, the face pale, the pupils contracted, the breathing slow and soft (although sometimes stertorous). An epileptic fit may now occur and terminate the scene, or the powers of life gradually become more and more depressed, and the victim perishes as if in a profound and gentle sleep. Now this progress and catastrophe, although viewed as evidence of an unmanageable, a malignant form of the disease, in a bad subject, is nothing more than the common course and result of injudicious management. Even Graves, who prescribed opium in delirium tremens in the manner I will afterwards notice, warns emphatically against its premature and incautious use.

"Opium," he says, "if given in the beginning, will increase the congestion and bring on sub-arachnoid effusion. I treated a case of delirium tremens in this way too boldly, and the man died with sub-arachnoid effusion; it was a lesson to me, and I advise you to profit by my experience."[2]

I am convinced that it is in this way very many of the sudden deaths we hear of in delirium tremens occur. I saw it frequently in early practice, and have seen it occasionally since in the practice of others; and feel persuaded that any practitioner who has been

[1] See also Case No. V.

[2] Clinical Lectures, vol. i. p. 530.

accustomed to treat this affection with large doses of opium, will be able, on reflection, to explain his want of success, and the occurrence of casualties. In fact, when recovery takes place after a long sleep forced on by a large opiate, it is simply from the wonderful conservative power of nature resisting the evil influence of the agent, just as some will recover from a severe apoplexy or a palsy. The practice is one of the utmost hazard. If death were the certain alternative in delirium tremens should sleep not be early obtained—for it it is said that "the patient must sleep or die,"—there might be some reason in attempting to force on the sleep by opiates. This, however, is certainly not the case, and consequently such interference is not only uncalled for but most improper, when there is danger to be apprehended from the practice. Sleep occurs as the natural, the favourable crisis, or rather termination of the disease; and it is not to be viewed as a part of the affection, or in the same light as we are accustomed to regard a critical sweat or other discharge. It is the result and the proof of an improved condition of the brain and nervous system,—a salutary relaxation succeeding a state of dangerous tension. It will take place in the mild but genuine forms of the affection at the proper period, which, as I have already remarked, is on the second or third day, when the paroxysm has run its course, when the peculiar erythism, the "nervous irritability," is brought to an end, and a condition of "exhausted nervous power" now truly produced. That sleep may likewise ensue in severe examples of the disease, although no opiate of any kind is given, the cases at the close of the present paper will prove; and while convinced that the plan of treatment now to be recommended will be found the most efficacious, I have no hesitation in saying that in a larger proportion of instances, sleep would take place spontaneously at an earlier period, and the subsequent condition of the patient be much more sound and safe, by doing nothing at all, than by the use of opiates. I have seen very decided cases of the disease recover well when a mere placebo was given with a view to keep up the appearance to friends of something being done, and prevent them from using as remedies things which would be hurtful. Dr Ware of Boston, in an excellent memoir on delirium tremens,[1] strongly advocates from experience the do-nothing plan of treatment. Among other things, he says of opium :—

[1] Quoted in the British and Foreign Medical Review, vol. xxiii. p. 603.

"In the cases which I have formerly treated with opium, and which have at last terminated well, a salutary sleep has not taken place till the close of the third day, let the quantity of opium be what it would. I have, indeed, seen sleep induced by opium at an earlier period, but it was premature, it passed into a state of coma, and the patient died. I am satisfied, therefore, that in cases of delirium tremens, the patient, so far as the paroxysm alone is concerned, should be left to the resources of his own system, particularly that no attempt should be made to force sleep by any of the remedies which are usually supposed to have that tendency, more particularly that this should not be attempted by the use of opium."

Dr Cahill[1] also cites several cases of the genuine disease, in which he found opiates decidedly injurious, and treatment without them salutary.

The treatment recommended by Dr Graves,[2] to which I have already alluded, is advocated on the ground that opium is highly dangerous in the early part of the paroxysm. His rule of practice is to begin with tartar emetic alone, with the view of combating vascular excitement, then to add a little opium, and gradually to increase the quantity, keeping its action carefully guarded and controlled by the antimony, until at length when engorgement of the cerebral vessels is no longer to be apprehended, to use opium alone. If opium is to be given at all in delirium tremens, this is certainly the safest mode of prescription. For some time I tried it, but from previous experience of the beneficial effects of antimony in this disease,[3] I soon became convinced that it was from that agent solely, especially its effects in the first stage, that ultimate benefit was derived; that the relative quantity of opium employed at first is too small[4] to counteract the power of the antimony, or to produce any notable effect whatever; that in ordinary cases, ere the time arrives for increasing much the amount of the opium, the

[1] Dublin Medical Journal. Observations on the Treatment of Delirium Tremens without Opium. Vol. xv. p. 397.

[2] Clinical Lectures, vol. i. p. 530.

[3] This experience of the effects of tartrate of antimony I had before I was aware that Dr Graves had recommended it with opium, or that Stoll, Göeden, Klapp, and others, had advised it in emetic doses.

[4] Dr Graves' formula for first use is:

℞ Antimon Tart. gr. iv.
Tinct. Opii. ℥i.
Aquæ ℥viij.

Signa. A tablespoonful to be taken every second hour. There is thus in each dose only five drops of laudanum to ¼th grain of antimony.

affection has run, or nearly so, its natural course, and the period for the salutary sleep commencing is at hand; and that when a greatly increased dose is given before this much wished-for change has arrived, there is a proportional increase of excitement and consequent delay of its occurrence.

From these considerations, I resumed the use of the antimony alone; and, during the last ten years, have treated upwards of eighty cases of the genuine disease, many of them very severe ones, with uniform success,—not only in regard to the speediness of the immediate recovery, but the comparatively thorough restoration to a healthy condition of body and mind;—as much so, at least, as could possibly be expected in individuals, many of whom had been, and were likely soon again to become, habitual drinkers. The dose which I have been accustomed to give has ranged from one quarter to one-half of a grain, in simple solution, every two hours, sometimes at shorter intervals, according to the degree of excitement and irritability. The action of the antimony appears to be chiefly sedative. Its direct influence is in reducing the vascular excitement of the brain, soothing the nervous system, and diminishing muscular power: and its more indirect action is exerted on the functions of the skin, kidneys, and intestinal canal. In two or three instances only have I found it necessary to suspend its employment, in consequence of diarrhœa and hemorrhagic discharge from the bowels; and in these cases digitalis and ipecacuan were substituted with marked benefit; and I do not recollect of ever seeing it produce continued vomiting, although occasionally I have found the first or second dose eject from the stomach a quantity of bile. It is for the sake of its emetic effect that, in Germany and America, it has been prescribed in large oft repeated doses, even from four to seven grains every hour, and that, too, according to report, with benefit.[1] But although there is, doubtless, extraordinary tolerance of this agent in delirium tremens, I do not think that the use of such, or any other very heroic means, are warranted. Bleedings, large opiates, or large doses of tartar emetic, are all, although certainly not equally, unsafe, and therefore to be deprecated. An antimonial course of treatment in moderation, and with the design I have indicated, gently diminishes excited action, induces weariness of muscle, general nervous exhaustion, and mental

[1] Quoted in the work of Höegh Guldberg, already referred to. See also British and Foreign Med. Rev., vol. vi. p. 330. Also Copland's Dict. of Pract. Med., p. 501.

languor. It thus removes all hindrances to the occurrence of the salutary sleep. It prepares the way for it, not by forcing, but by favouring it; and when the individual, exhausted, seeks his couch, and finds repose, that goes on, not as a drugged sleep, but as a purely natural and profound repose, from which he awakes with restored reason and muscular control.

Although I have recommended the tartrate of antimony as a chief remedy in delirium tremens, there are several other means essential to its successful treatment. In the department of medicinal agents, however, I have only further to suggest, that, should the bowels not be moved by the antimony, the compound powder of jalap (ʒi) will generally be found speedy and efficacious. The other means of cure belong strictly to regimen and diet; and the first of these in importance is bodily freedom. Nothing is more hurtful in delirium tremens than restraint, particularly that of the strait-waistcoat. I have seen instances, and heard of many more, where the cerebral excitement was so increased by the never-ceasing maddening struggles for liberty, that fatal convulsions at last afforded release. All the control required is the presence of one or two judicious attendants, who will humour the patient in his whims and fancies; who will speak and act regarding them so as to assure him of safety, and to relieve him of apprehension, which is the most characteristic feature of the delirium; and who will mildly but firmly interpose, if he attempts anything which may accidentally prove injurious to himself or others. Of course injury inflicted wrathfully or vindictively is not to be anticipated, for rage, violence, or outrage, do not occur in this remarkable disease, but only in that affection which I have already briefly noticed, and with which it is sometimes confounded, namely, the madness of drink. Hence the frequent accounts met with in the public prints, of homicidal, suicidal, and other violent acts, said to be perpetrated during fits of delirium tremens, originate in an entire misapprehension of the nature of the two diseases. The apartment, however, in which the delirium tremens patient is confined should be well secured, for he may rush out at the door, or jump over the window, in the fright and frenzy of imagined danger. The larger, too, the room is, so much the better, that he may have ample space to advance and retreat, according as he wishes to scrutinize or avoid a suspicious or distressing object of his fancy; to arrange and re-arrange articles of furniture; or to carry on, after a fashion, the duties of some bustling occupation.

All this expenditure of muscular effort, without any restraint, aids greatly the antimony in producing a safe kind and amount of physical and mental exhaustion, from which the patient, languid and worn out, at last lies down voluntarily, and falls into the much-desired sleep. It is thus, too, that "the walking drill," according to Dr Blake's experience in the West Indies,[1] was found efficacious in warding off attacks of delirium tremens in the case of drunken soldiers; not, however, as supposed, from the exercise proving a new stimulus in place of the rum, to which they had no access, but from its wearing-out effect, while the proper nutrition of the body was maintained. No one would ever think of ordering continued and monotonous hard work, and muscular fatigue, for an affection of "exhausted nervous power."

During the entire paroxysm of the attack, it is of some consequence to afford the patient abundance of light; not, however, as supposed by Dr Blake,[2] on account of its stimulant or excitant effect, but for its aid in correcting false optical impressions. The excited brain is very apt to receive erroneous impressions from the appearance of surrounding objects, if there is an uncertain light. Hence the exaggeration of many of those agitating and terrifying illusions and phantasms which more distinct vision would prevent, or quickly dispel. During the daytime, therefore, there should be no half-closed shutters, nor half-drawn blinds or curtains, but advantage taken of the clearest light available; and during the evening or night, the more distinct the artificial light is so much the better. Perhaps perfect darkness may serve the purpose equally well; but this can be available only in the well-padded chamber of a lunatic asylum; and, besides, in private practice, the other parts of the plan of treatment here recommended, which requires the presence of an attendant to regulate the doses of antimony, or other sedative, and to administer, from time to time, suitable nourishment, could not be carried on without the admission of light. This leads me to remark, in conclusion, that, during the administration of the tartar emetic, I give, at intervals of a few hours, a moderate quantity of good beef-tea, mutton broth, or chicken soup, and sometimes *café au lait*, with the white of an egg switched up with it. Thus, while the vascular action in the brain is being subdued, and the nervous system liberated from the presence of the alcoholic poison, the functions

[1] Op. cit., p. 19. [2] Op. cit., p. 60.

of organic life are sustained, and a better ultimate recovery is secured.

I shall now conclude this paper by appending reports of nine cases, in order to illustrate the views advocated in regard to the nature and treatment of delirium tremens, and these I shall give in detail, so that it may be acknowledged that the true disease has been understood and described. These cases, too, it must be borne in mind, are not complicated, or the symptoms masked by the effects of stimulants and opiates—unless in the instances of the unauthorized supplies hereafter to be noticed. They may probably, therefore, on first consideration, not appear to be remarkably severe instances of delirium tremens. They are indeed, however, most characteristic examples of the affection,—five of them at least being considerably above average severity; and I have no hesitation in stating my conviction that all of them would have assumed a more severe complexion and taken a more serious course, had the ordinary remedies been employed; nay, that it is the stimulo-narcotic plan of treatment alone which makes this disease in almost any case appear a formidable one. The following cases also are not selected; for, with three exceptions, they all occurred consecutively at a period —some years since—when I purposed bringing my views before the profession; and the others have occurred in succession also, and were noted with a view to publication; and, but for extending this communication to an undue length, I could have given the details of many more examples of the disease and its treatment.[1] Enough, however, I trust, has been brought forward to show, that delirium tremens is a form of alcholic poisoning—or an alcoholism; that its exciting, as well as its predisposing cause is the habitual abuse of intoxicating liquors—and not the sudden abstraction or diminution of accustomed stimulants; that these produce a specific form of irritation of the brain and membranes, the tendency of which is to arachnoid inflammation; that this takes place most readily in those who have a highly sanguine temperament, and a nervous irritable disposition; that the chief phenomena attending this disease are invariably uniform in their character, and distinguish it from every other

[1] As the individuals, whose cases are noted in Nos. I., II., III., IV., and V. were affected repeatedly with delirium tremens before or since the occasions described, amounting in all to twenty-three times; and as they were all these times successfully treated on the plan now recommended, the cases now brought forward may be considered as thirty-two in number, instead of nine.

affection; that the occurrence of the salutary sleep is the normal termination of the paroxysm, indicating diminished activity of the cerebral circulation and functions, and the commencement of convalescence; that the cordial and opiate treatment is generally pernicious, and frequently dangerous; and that the main indications of cure, are, to reduce the cerebral excitement by a moderate but decided and steady course of antimony, or other agent capable of exerting a somewhat similar influence, and thus favour—not force—the wished for sleep; to soothe the feelings and dissipate the fears of the affected by kind and judicious superintendence, and the permission of light and liberty; and to support the physical strength by a moderate allowance of animal nourishment.

CASES.

Case I.—An innkeeper, aged 48.[1] A habitual drinker, but seldom or never intoxicated, has been attended by me in eight different attacks of delirium tremens—more or less severe—within six years; and on all the occasions treated successfully without stimulants or opiates. For some time previous to the present attack he is said to have been drinking less than in former years, in consequence of ill health, and still less within the last fortnight, although every day, early and late, imbibing a little with his customers. On my *first* visit I found him very dull; without appetite; sleepless; complaining of slight cough and pain in the chest; tongue slightly furred; bowels constipated; pulse 90. As yet no visions, and no decided tremors. Calomel gr. iv. Pulv. Jalapæ, comp. 5i., ordered.

2nd day.—Bowels slightly opened; cough troublesome, but no bronchitic signs. In other respects same as on previous day. On account of the catarrhal symptoms ordered a mixture every four hours, each dose containing, Sol. Mur. Morphiæ ♏ 5, and Liq. Acet. Ammon. 5ij.

3rd day, noon.—Paroxysm of delirium tremens evidently begun. Had passed a restless night. There is considerable tremor and agitation of manner. Pulse 94, full, but soft. Discovered that he had been getting a little wine and spirits from time to time during the last three days. Forbade everything of the kind, and withdrew also the mixture ordered yesterday.

4th day, 10 A.M.—Pulse 106, soft and small; hands and tongue very tremulous; face pale; perspiring copiously. Could not stand still for a mo-

[1] For obvious reasons the names—even the initials—of the individuals, whose cases are detailed are not given, or the dates of attack mentioned.

ment, but darted from one window to another, as he anxiously expected the police to come for thieves, who, he said, were tied up in the next room. They had been stealing his property for the last six days, and he had just been writing down a list of the articles amissing. This inventory he showed to me, but it was utterly unintelligible. He knew me at once; and answered correctly questions when this could be done in four or five words. It appeared that he had been in bed, but was sleepless, tremulous, and agitated, until 3 A.M., when he was seized with what his wife described as a fit. She then gave him a wine-glassful of brandy, and ever since he has been up and much excited,—running about the house after imaginary rats and thieves, and once escaped to the street in his night-shirt publishing his wrongs. A trustworthy attendant was now placed over him with instructions to see that he had as much freedom and light as possible. All stimulants prohibited, and the following mixture ordered: —℞ Tart. Ant. gr. iv. Vini Ipecac. ℥iij. Aquæ ℥vij., ℥v.—a table spoonful to be given every two hours. A cup of tea, café au lait, or beef-tea, to be given at a few hours' intervals, if cared for.

8 P.M.—Pulse 96. Bowels twice moved since the morning; perspiring copiously; and much calmer, although still talking in a rambling manner concerning all sorts of difficulties and troubles. A cupful of beef-tea twice taken. The antimony has been given regularly, and is to be continued until there should be an appearance of depression, and a wish to go to bed.

5th day, 11 A.M.—He had his last dose of tartar emetic at ten o'clock last night, and soon thereafter appeared exhausted, and was prevailed on to go to bed, when he slept profoundly from 11 P.M. to 5 A.M. He had then some tea and bread, and had been sleeping again until now. Is dull, but quite rational; pulse 70, soft and regular; skin moist; bowels once purged. To be kept very quiet, and to have nourishment as formerly directed.

6th day.—Had slept the most of previous day, and all the last night; is now quite convalescent, although weak.

Remarks.—A good example of an ordinary case of delirium tremens; reduction of accustomed stimulants from inability to take more; aggravation of all the symptoms from (unauthorised) administration of stimulants, and probability that the disease would have taken a more severe form had these been continued; early and decided improvement under treatment with antimony, animal nourishment, and careful watching; the patient a living proof of the safety of a non-stimulant and non-opiate treatment from good recovery under so many attacks of the disease—the more frequent the recurrence of delirium tremens, the greater being generally supposed the danger.

CASE II.—A spirit-dealer, aged 48. Long an habitual drinker. His average daily amount for some time had been four gills of whisky and one bottle of beer, taken from early in the morning until late at night; and there had been no diminution in the quantity previous to the present seizure. Had

slept very little for a week, and none at all on the last two nights; and for some days was very tremulous, and quite unable to transact business.

1st day's visit, 3 P.M.—Was very distressed and agitated during the last night,—walking constantly up and down through the house, terrified with visions; had his last glass of whisky at 11 this forenoon. Pulse 104, small; skin cool and clammy; great muscular tremor; tongue foul; eyes yellow and lustreless; mind constantly occupied with false and horrific impressions of all kinds, although in no very definite form; but can answer a question put directly to him. *Instructions*—Plenty of light; complete liberty to promenade through the house, the doors and windows being secured; and two intelligent men to attend and humour all his fancies. To have a wine-glassful of the following mixture every two hours:—℞ Tart. Ant. gr. iv., Infusi Quassiæ et Aquæ ā ā ʒx., whether it sickened or not, and only to be discontinued if he should go to sleep. Beef-tea and weak coffee with milk to be given occasionally. 8 P.M.—Took one glass of the mixture at 3.30 P.M., which caused vomiting of a quantity of bilious matter; one at 5 o'clock, which was followed soon after by a loose alvine evacuation; and one at 7 o'clock. He is at present pale and perspiring; very tremulous and restless—in constant apprehension of rats and strange men; quite sensible when spoken to; pulse 110. To have the mixture only every third hour. Beef-tea, etc.

2d day, 10.30 A.M.—Pulse 106, very small; perspiring freely; face very pale; urine scanty and high-coloured; great tremulousness. He can put out his tongue, or rise up, or sit down when desired, but that is nearly the amount of his intelligence. He is in constant motion, not rapid or boisterous, but chiefly busy in arranging bed-clothes, carpets, small articles of furniture, and sweeping imaginary crumbs from off the table. Had never been in bed, and had taken only three doses of the mixture since I saw him last. Took a glass from me, supposing it to be pale brandy:—no sense of taste. The mixture to be continued regularly. Was seen by my friend Dr Cappie at 3 P.M., and again at 9 P.M., who found him much the same as when last reported. Had been purged several times. Antimony, etc. continued.

3d day, 2 P.M.—In bed, sound asleep; pulse 84, of good character; a good deal of subsultus tendinum; skin very moist; paleness of countenance gone. It was stated that he had appeared very much exhausted last night about 12 o'clock; was then got to bed, fell asleep almost immediately, and did not awake until 7 this morning. When awake he was not quite sensible, but took some bread, coffee and milk, and fell asleep again. Continued so for other two hours, and was then perfectly coherent, but not inclined to speak. He had some more breakfast and an egg, and went to sleep again. An hour ago he was awake for a few minutes, and took some beef-tea. The antimony had been given once this morning:—to be discontinued. Nourishment only to be offered when he awakes.

4th day.—Found him quite well; mind perfectly clear, and had been able to read a little.[1]

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[1] This patient has since had another attack—not quite so severe, originating, without any diminution of habitual supplies, and successfully treated in the same way as on the former occasion.

Remarks.—An ordinary case of the disease, rather more severe than Case I.; no suspension of wonted libations up to the period of seizure; excellent illustration of the *modus operandi* of the tartar emetic; and also of the benefit derived from the other means recommended for sustaining the organic functions, and bringing about natural sleep.

Case III.—An engineer, aged 30. Had been twice formerly under my care in delirium tremens, and recovered well without the use of stimulants or opiates. Has been drinking largely and constantly for some months past, and exhibiting at times excessive irritability and violence of temper—even to the extent of threatening the lives of his wife and children. In apprehension of this disposition he was some time since treated for delirium tremens in the Morningside Asylum; but on what plan I do not know, save that he was confined in a dark chamber. On the present occasion, at my *first visit*, I found that he had been drinking up to the moment of his attack, which had commenced decidedly two days before. His pulse was 110, soft, and of tolerable strength; hands very tremulous; aspect extremely haggard; skin moist; tongue clean. He had been quite sleepless for two nights; but not violent in his manner or conduct. He was laughing and talking incoherently,—looking constantly under his pillow, and carrying on a conversation with imaginary beings underneath, in this way,—"aye, oh yes, yes, certainly, just so," etc. On requesting to know what the devils were wanting, he replied, "a glass of whisky." Prescribed—℞ Tart. Ant. gr. vj. Aquæ ℥ij. Solve. A wine-glassful every two hours; and desired that he should be closely watched, and kindly treated. To have some weak beef-tea occasionally.

2d day.—Had a few minutes sleep this morning, but his general aspect is in all respects worse. Pulse 116. He is very restless and agitated, wishful to get out; thinks his workshop is on fire; that the police want to get hold of him, and has many such like fancies. I discovered that a bottle of table-beer had been given to him this morning. A wine-glassful and a half of the antimonial solution to be given every two hours.

3d day, 11.30 A.M.—He had walked about all yesterday in a state of great excitement; got the antimony very regularly, and lay down for the first time about 10 P.M., when he fell into a sleep. This continued until 6 A.M., when he awoke quite collected, and has since continued so. Pulse 80; hands very tremulous; has taken a good breakfast, and is in all respects apparently convalescent.

4th day.—Quite well.[1]

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[1] Since the above occurred, this patient, on account of domestic calamities, was removed to the Royal Infirmary, under another attack of delirium tremens, where he died in an epileptic seizure. I understand that the plan of treatment practised in that instance was the one usually followed—namely, restraint in a strait-waistcoat, stimulants, and opiates.

Remarks.—A well-marked case of delirium tremens occurring in an individual in whom the *delirium ebriosum* might rather have been expected; but the long-continued course of intemperance gave to this attack the usual characters of cerebral alcoholism. The case also shows the liability to this disease without diminution of the wonted stimuli, and the tendency to aggravation even from a slight stimulus, such as table-beer; it also illustrates the singularly sedative effect of antimony.

Case IV.—A gentleman, aged 40, of highly sanguine temperament and nervous, but kind disposition. Has had two previous attacks of delirium tremens under my care, both very severe. Was a total abstainer for some time following his last illness, having been informed that he would not probably survive another attack, or if so, that in all likelihood he would become insane; but had been gradually led back to drinking habits through company. His digestion having soon become so impaired that he could not take substantial nourishment, he drank systematically to overcome the distressing sensations of sinking. Four days prior to my first visit he was unfit for business; had disturbed wakeful nights; was very tremulous and nervous—being aware that delirium tremens was approaching, and fearing that he might be deprived of reason; and he had lost entirely the appetite—even for drink. Some brandy, however, had been given to him on the previous day, and his bowels were well cleared out by laxative medicine.

1*st day, afternoon.*—Found him very much agitated, and talking quickly and incessantly. He was hearing sounds which reproached him as a bad man. Thanked me for visiting him, saying that he was undeserving of notice, having been such a rogue, and so cruel to his wife and children. Pulse 100, soft and full; skin warm and moist; countenance pale and anxious; tongue and hands very tremulous. ℞ Tart. Antim. gr. iv., Aquæ font. et Aquæ Cinnam. ā ā ʒiij. Sig. One tablespoonful every two hours, or oftener should there be more excitement.

2*d day*, 10.30. A.M.—Had several times last night a few minutes' sleep; pulse 104; tongue white. He is at present more excited and restless; looks on himself as a lost man; is constantly hearing strange sounds; and is every little while eagerly examining the corners of the room. An experienced male attendant now placed over him with usual instructions. Antim. continued.

8 P.M.—Much the same. Mind more agitated with regret, and the apprehension of some impending calamity.

3*d day*, 10 A.M.—Has spent a sleepless and agitated night. His aspect is very anxious and apprehensive. Considers himself "between the deep sea and the devil; in fact, too bad for the devil himself." Pulse 108; tremor great; tongue cleaner; bowels confined; sweating considerably at times; urine scanty and high coloured. Tartar emetic continued; and to have Calomel gr. iv. Pulv. Jalapæ Comp. ʒi.

7 P.M.—Pulse 110, smaller than formerly; bowels have been twice opened. He is at times moving rapidly about as if searching for something; now whis-

pering as if aware of some secret; and then again standing gazing at the floor bowing and scraping, and answering questions as if before a high tribunal. Already 12 grains of the tartar emetic have been given. To have now half-grain doses every two hours, or oftener if he becomes more excited. Also to have some beef-tea occasionally.

4th day, 11 A.M.—Had passed a violent night; fancied robbers were in the house, etc., and occasionally he shouted so loud as to be heard in the street. Had a large basinful of beef-tea at one time, and some coffee and milk at another, during the night. Between seven o'clock last night and seven this morning he had taken 6 grains of antimony. Shortly after the last dose he fell asleep, and has slept until now—four hours—awakening quite sensible. His look is now free from apprehension, but haggard and as if worn out. Voice and manner wonderfully firm. Pulse 96, and of good character. To have beef-tea, and no more antimony.

8 P.M.—Is again much worse. He is lying on the sofa sullen and dejected, with a very maniacal aspect, and declaring that he was the worst of men and doomed to die. It appeared that he had gradually been getting worse since the forenoon. His keeper and friends supposing him beyond risk, had allowed several acquaintances to visit him; and it was my firm conviction at the time, that a stimulant had been administered by some one. The half-grain doses of tartar emetic were ordered to be renewed.

5th day, 11 A.M.—Had wandered from room to room all the past night. Thinks that the newspapers contain a great deal about him, and that various enemies are plotting his destruction, etc. Pulse 98, firm; tremor inconsiderable; perspiring freely; bowels opened twice during the night. Antimony to be continued.

10 P.M.—Was very calm about noon; sat for some time in the parlour with his wife, reading the newspapers, and kissed the children. He then again became much excited. I found him standing in an attitude and with an expression of reverential awe, arms extended, body slightly bent forward, and eyes turned upwards. His language was as if answering questions put by his Maker, such as, "Yes, Almighty God."—"Dr Peddie cured me of my fever, Almighty God."—"M.D., Almighty God," and so on. Having sat down for an instant I was implored with an expression of deep alarm and concern to get up, otherwise I would be killed on the spot. Pulse 118, not so firm as in the morning; skin moist; tongue pretty clean; bowels twice moved. Had taken tea and some soup. The antimony to be continued.

6th day, 10 A.M.—Had passed a very restless night, having had altogether only about half an hour of sleep, from which he has newly awoke. Lying in bed with his clothes on, pretty calm. He said, "Don't speak to me, as you little know what a bad man I am. The sooner I am out of the world the better, for I am to be publicly whipped through the streets to-morrow morning," etc. Pulse 92; not much tremor; tongue a little furred. Bowels repeatedly purged. On account of this looseness I withdrew the tartar emetic and substituted—℞ Vini Ipecac. Tinct. Digitalis, ā ā 3ss., 20 drops to be given every three hours.

9 P.M.—Had been much much calmer since 2 P.M. The last mixture to be continued.

7th day, 10.30 A.M.—Had slept soundly from ten last night until nine this morning. Is still gloomy and desponding. Digitalis, etc., to be continued at intervals of eight hours, and as much nourishment as he will take.

8th day.—Passed a good night, and is in all respects much better.

From this period he gradually improved. Sleep became more and more refreshing, and the mind stronger. In a week he was able to resume all his ordinary occupations; and has continued ever since—after a considerable lapse of time—temperate and well.

Remarks.—This case is an example of true delirium tremens at the outset, modified somewhat in the relapse, with symptoms of a maniacal character occurring in an individual apparently so predisposed, but not of the nature of delirium ebriosum. This maniacal relapse was in all probability occasioned by some improper tampering. It shows also the benefit derivable from ipecacuan and digitalis, when the tartar emetic begins to purge too much.

CASE V.—A journalist, aged 41. Had for many years been in the habit of Morphia eating. The ordinary quantity consumed was 9 grains of the solid Muriate per day, or 5i. in the week. Had been under my care (over a period of several years) during seven previous attacks of delirium tremens, most of them severe, and on all occasions treated without stimulants *or opiates*. After each attack he abstained for a few days or weeks from morphia, but the necessity of fulfilling some literary engagement drew him again into the vice. His whole appearance was that of the confirmed opium eater, yet there never had been any tendency to delirium tremens so long as stimulants were abstained from. The occasions in which he indulged in intoxicating liquors to any extent, only occurred at intervals of many months. Begun by the excitement of the social board on some festive occasion, they continued for from two to three weeks, but never so as at any time to produce decided intoxication. The quantity generally taken amounted to three wine-glassfuls of spirits, and one pint of porter daily; and this short course of drinking invariably led to an attack of delirium tremens. While indulging in liquor, he had always found it necessary to diminish a little the habitual dose of morphia, on account of the nervous irritability and tremor which he soon began to experience.

1st Visit, 5.30 P.M.—Was made aware that the course of cause and effect has been the same on the present as on former occasions; and that for the last four days, feeling himself under all sorts of horrors and fancies, and unable to sleep, he had left off the morphia entirely. Last night, however, he had taken three grains, having been induced again to try if it would produce a composing effect, but instead of this, it made him, as he himself expressed it, "ten times worse." He is at present walking up and down in a most wretched condition. States that strange visitors had been all day talking to him in whispers; that his breath as it went in and out took the form of whispers accusing him of misconduct, which, he says, is little needed, as his conscience is sufficiently reproving. Feels as if he had two heads—the one conjuring up fancies, and

the other thinking and judging correctly. Asps are also crawling on his breast, and he cannot shake them off, etc. Pulse is 100, soft, and of very good strength; pupils contracted; tongue clean; no appetite. Tremors, not only of his hands and tongue, but of his head and whole body have much increased to-day; and his voice also is bleating and unsteady. Says he has slept none for some nights, and is much afraid of the one approaching. He also states he had vomited himself freely with some antimonial solution which he had beside him since last illness; and likewise that he had been well purged. Desirous of ascertaining whether any other depressing or sedative agent would answer as well as the antimony formerly employed, the following was prescribed:—℞ Tinct. Aconiti gtt. x., Aquæ ℥iv.,—a tablespoonful to be given every two hours, and some beef-tea occasionally.

2d day, 10 A.M.—Has spent a very restless night; had some short, but very disturbed sleeps. Asps and other reptiles are crawling in great multitudes about the bed. Appearance of countenance most wretched; skin slightly clammy, and of a dirty colour; tongue a little white; pulse 110, weaker than yesterday; urine scanty, and high-coloured; no appetite. Had taken during the course of the night one-half of the mixture (Aconite 5 ♏), but being unable to visit him frequently, so as to watch the effects of the medicine, and fearing it might prove too depressing for the circulation, I withdrew it, and ordered instead 10 drops of Ipecacuan wine, to be given every two hours, and ʒi. of the Pulv. Jalapæ Comp. at noon. To have also beef-tea.

4 P.M.—Much more excited and delirious; says there is a court sitting in judgment on the five senses, etc. Tremors are excessive, and he is sweating profusely. He confesses to me that his attendant, thinking him very weak, had at noon given him a wine-glassful of spirits. And on close questioning, also admits that he has some morphia in his possession, but will not say whether he has taken any, and will not give it up. To have 20 drops of the Vin. Ipecac. every two hours.

10 P.M.—Is rather better. Pulse 108. Says that he has had a great many strangers visiting him, pressing him very much to go out with them. Pupils contracted.

3d day, 10 A.M.—Has passed a most agitated night; walking from room to room incessantly; sometimes waving a white handkerchief from the window, under some notion of making peace with God. Has taken a little breakfast. He had gone to bed a little before my visit, and is lying with his eyelids half-shut, and squinting when they are open; is working with his hands in the air, as if arranging things, or seizing objects between the finger and thumb, or pointing a way. There are great general tremors, and considerable twitchings of the eyebrows. Pulse 110, and very weak. To have plenty of good beef-tea; also the Vin. Ipecac. to be continued.

4 P.M.—Pulse 114. Is sitting up in bed, very pale, and talking more humorously. A lady and gentleman, whose portraits are hung on the wall, have been speaking to him from out of their frames, and annoying him much, but he had discovered that by a particular wink of his eye, he could make the one jump into the frame of the other, and thus stop their discourse. His attendant delivered to me a small packet of morphia which was found in his

possessiou, a seareh having been made for it by my instruetious ; and he did not deny that he had been helping himself to a little from time to time. Ipeeae. to be eontinued ; also beef-tea, ete.

4th day, 10.30 A.M.—Had spent a very turbulent night. Is lying at present with a poker in his hand, with whieh he had been warding off supposed intruders. Now, however, he is asleep, and has continued so sinee 9 A.M., but is very distressed, judging from his moans, and the movements of the museles of the forehead and eyebrows.

2 P.M.—Has just awoke after sleeping five hours. Pulse 84 ; hands very tremulous, and voiee bleating; tongue eleauer. Mind eonfused, but no raving. Inclined to be quiet. To have beef-tea or a mutton ehop, if he eau take it. Diseontinue medieine.

5th day, 10 A.M.—Has passed an exeellent night. Pulse 80, soft, and of good character. Feels his head quite eool and elear ; and talks intelligently on some favourite subjeets of study.

6th day.—Greatly improved. Intends to walk out a short distanee.

7th day.—Feels better to-day than he has done for months.[1]

Remarks.—The above ease is an illustration of the opinion that morphia alone (or opium), will not produce delirium tremens, but that the eombination with stimulants will very readily induee an attack—even a very short eourse of drinking. Here also it is apparent, that the morphia latterly produeed a stimulating and injurious effeet, so mueh so, that during the premonitory stage of the delirium tremens, the patient voluntarily diminished the dose ; and that subsequently, its stealthy use protraeted and aggravated the attaek. I am of opinion that the ipeeaeuan did good (although, perhaps, not so mueh as the tartar emetie would have done); and that its effeets would have been more apparent had spirits not been taken on the second day of the paroxysm, and morphia repeatedly.

Case VI.—A elerk, æt. 41. For a number of years a hard drinker, and for some time past indulging to a great exteut, but said to have been very moderate during the last six or eight days, in eonsequenee of geueral indisposition. Appears to have twiee had delirium tremens. On the last oeeasion is said to have been very violently and dangerously ill for three weeks, and to have been treated with spirits and opiates.

1st day.—Pulse 98 ; tongue foul and tremulous, also eonsiderable tremor of hands, and agitation of manner ; eouutenanee anxious ; bowels mueh disordered ; sleepless. To have ℞ Mass. Pil. Hydrarg. gr. iv., Ext. Coloeynth. gr.

[1] This patient died sometime sinee apparently from an apopleetie effusion, without having been under any form of treatment. Upwards of two years had elapsed sinee the attaek above described. The praetiee of morphia eating, however, was soon resumed, and was persevered in to the last.

vi. Misce ut fiant, pil. ij.,—for immediate use ; and afterwards 30 drops of the Vini Tart. Ant. to be taken every three hours.

2d day, 6 P.M.—Was unable to see him earlier. Pulse 98, full, but soft ; bowels have been freely purged. Slept none since last visit ; has been very restless all day ; and being himself apprehensive—from previous experience—of the approach of delirium tremens, has ordered the windows to be nailed down, and his razors removed. Antimonial wine to be continued. 9 P.M. Is now seeing objects in bed, and becoming more restless. Pulse 100, of good character. Tongue cleaner. Now to have the antimony in larger doses. ℞ Tart. Ant. gr. iv., Aquæ ℥ij.,—a wine-glassful to be given every two hours. Arrow-root and coffee with milk allowed. To have liberty to walk about the house; to have all his opinions and diseased impressions humoured as much as possible ; and to have the advantage of clear light.

3d day, 11 A.M.—Says that he has had eight hours of excellent sleep, and feels quite well and comfortable. In reality, however, he has spent a very restless night ; up and down through the house several times with a lighted paper, looking for thieves. Has been much quieter since the last dose of the antimony. Pulse 106 ; more tremor ; considerable warmth and moisture of skin. Antimony to be continued, and beef-tea to be given occasionally.

4th day, 10 A.M.—Pulse 110, weak, and slightly intermitting ; muscular tremor great ; perspiring copiously ; pupils large ; face pale ; urine scanty and high coloured ; bowels open ; no sleep ; and very much excited with all sorts of fancies, although he can answer any question distinctly. 8 P.M.—In most respects much as he was in the morning. Pulse 120 ; skin clammy. He has been travelling all day along with his wife, and something or other has constantly been going wrong. Now, they are both (his wife to please him) sitting in bed with their knees drawn up to prevent water covering them, as it has got into a boat in which they are crossing a river, etc. To continue the antimony, etc.

5th day, noon.—Pulse 100, and steadier. Tremor not quite so great. Has taken some breakfast with relish, and more beef-tea. Has been in bed several times over night, but has only had one hour's sleep. At present is doing penance by walking on the floor barefoot. To continue the antimony, etc. 4 P.M. Is much calmer ; pulse down to 86. In order that fatigue and consequent sleep might be produced, advised a half-hour's walk out of doors in charge of a friend. 7 P.M. Sent for hurriedly. He is much more excited—more than he has been for the last 24 hours ; pulse 115 ; said that he saw his wife disposed of at a lottery a few minutes ago to another gentleman, which he considered most disgraceful, etc. He had been out walking for a very short time only ; and although I suspected that liquor had been given to him by some one, I could not ascertain that this was a fact. Antimony now to be given in half-grain doses every two hours.

6th day, 11 A.M.—Has passed a very agitated night. A short time since made an attempt to get over a window, and knocked down a large flower box into the street in the attempt. Is now writing dispatches to the Duke of Wellington, as he thinks himself in a besieged fortress. The writing is mere scratches of the pen, no letters being formed. Pulse supposed to be about 120,

but the muscular tremor and tendinous jerkings are so great as to prevent its being correctly counted. Half-grain doses of antimony to be given every hour, and beef-tea liberally. 11 P.M. Shortly after last visit the excitement had begun to subside, and with each successive dose of the tartar emetic he was observed to become calmer and more rational. At 3 P.M. he was taken out by a hired keeper for a short walk, but the latter having shortly before had some drink, took his charge into a spirit shop to get more. Here the patient, although offered liquor, declined; but the attendant having drank freely, and being unable to take care of himself, the former had actually to help him home. Worn out by this exertion, he went immediately to bed—half-past 4 P.M.—and has slept soundly until now. He is quite composed and rational. Antimony discontinued.

7th day, 11 A.M.—In all respects quite convalescent. Had eight hours sleep over night.

8th day.—Left cured.

Remarks.—The above is an instance of a somewhat severe attack of delirium tremens, in which recovery took place under the use of tartar emetic, etc., at a much earlier period than on the previous occasions when the treatment was stimulo-narcotic. It is also an additional illustration of the fact that the accustomed stimulus is frequently diminished by the patient himself when the disease is forming and advancing, simply from a sense of inability to stand the same amount as formerly. It likewise illustrates what I have frequently observed, namely, a severe outbreak of excitement shortly before recovery commenced, but which must be unhesitatingly met with the antimony, perhaps in increased doses.

CASE VII.—The last-mentioned patient having fallen back into habits of intemperance after the lapse of a year, was again seized with delirium tremens. On my *first visit at* 5.30 P.M., I found that he had been carefully treated and watched by an intelligent student of medicine who lodged in the same house, and who not only had observed the success of the practice pursued on the previous occasion (Case VI.), but had during the interval, by the timely use of the same means, cut short two threatened attacks of the disease. It appeared that he had been drinking very freely up to the premonitory period of this attack, three or four days ago, since which time he has had an aversion to liquor, has tasted none, slept none, and laboured—especially during the last two days—under considerable excitement of the usual kind. There had already been given to him, in one-fourth and half-grain doses, viij. grains of the tartrate of antimony. His pulse I found to be 120, weak and slightly irregular; skin clammy; hands tremulous; face pale, notwithstanding a considerable eruption of acne; pupils dilated; and the tongue furred and presenting prints of the teeth along its edges. His chief occupation was examining anxiously the corners of the room, presses,

etc., arranging articles, partly real, partly imaginary, and taking memoranda, which, however, were quite unintelligible. Advised a continuance of the antimony.

2d day, 1 P.M.—Pulse 120; considerable heat of head and skin, but with general moisture. Answers any direct question rationally, but immediately thereafter talks incoherently. Is greatly occupied looking after dogs and cats, who, he says, have got into the house. The urine is scanty and high coloured, coagulates readily with heat and nitric acid; and a drop evaporated spontaneously presents numberless beautiful phosphatic stellæ. Antimony to be continued; and beef-tea to be given occasionally.

10 P.M.—Pulse 110. Is not quite so agitated and restless, but still loquacious and fanciful. Has hitherto had the antimony at the rate of 4 grains in 12 hours:—to be continued at the same rate, and good beef-tea to be given from time to time.

3d day.—Did not see him to-day, but in the evening received the following report from my medical friend in attendance:—"After you left last night he talked for a little, but quieted down about 11 o'clock, went to bed shortly afterwards, and slept soundly until 5 o'clock this morning. On awakening he took some of the antimonial solution, drank freely of barley water, and fell asleep almost immediately. He awoke again at 8; and has had short naps during the course of the day. He complains a good deal of headache, and his eyes are dull and heavy, and water a good deal. There is a copious flow of urine, which is much lighter in colour. Pulse 88. No excitement or delusion, and he is inclined to sleep. I have been giving the antimony, but not so frequently of late."

4th day, 11.45 A.M.—Is quite convalescent but weak; urine still somewhat high coloured, but is unchanged by heat, although rendered slightly turbid with nitric acid; and on evaporation no phosphates are discoverable, but a large amorphous deposit of the urate of ammonia.

Two days subsequently I found him up and going about quite well, and the urine perfectly normal.

Remarks.—The above case is a well-marked instance of delirium tremens in which stimulants were freely taken up to the occurrence of its premonitory stage, when they were suspended in consequence of the constitutional effect of alcoholic accumulation having been established. The easy course which the disease afterwards ran, compared with that of the previous attack, was undoubtedly attributable to the prompt and decided use of the tartrate of antimony. The case is also interesting from the circumstance that during the paroxysms of the affection, the urine was found to be highly albuminous, and loaded with phosphates, a condition which is probably invariable in this disease.

CASE VIII.—A wine and spirit merchant, æt. 42, long addicted to drinking

habits, not consuming much at one time, but imbibing upwards of a pint of spirits daily. He was attended by me, about a year before, in an attack of delirium tremens of considerable severity, but recovered well under the antimonial plan of treatment. There did not appear to have been any diminution in the amount of his supplies on the present occasion, but rather the reverse, up to the moment of my *first visit*, which was *at* 4 P.M. I found him in his shop, to which he had escaped from home, although apparently in the second day of the paroxysm, extremely excited, and busily engaged among boxes, bottles, and barrels, searching for lost articles. The delirium was of a very rambling and confused character, and his hands and tongue excessively tremulous. Pulse 110, and small; skin very dry; pupils large; face pale and having an anxious expression. Prescribed ℞ Tart. Ant. gr. vj. Supertart. Potassæ 5i, Aquæ ℥ij—a wine-glassful to be taken every three hours, or oftener, if there be more excitement. To be taken home and closely watched, etc.

2*d day*, 11 A.M.—Pulse 114. Tremors greater and more general, as if he was paralytic. Urine high-coloured and scanty. Has been wandering through the house all night in an extremely excited state, and, so far as was intelligible, under the apprehension of visits from burglars. Antimony to be continued every two hours, and ℥viij. of strong beef-tea every six hours.

3*d day*, 11 A.M.—Pulse 80, and of good strength; tongue clean; urine like strong beer in colour, but more plentiful. Manner calm; mind collected; and altogether apparently quite convalescent. It was stated, that yesterday, about 4 P.M., he fell asleep, awoke about 6 P.M., took some beef-tea, immediately fell asleep again, and did not awake until 10 P.M. He then had more nourishment, and was again soon fast asleep, in which he continued until 6 o'clock this morning, when he awoke and took a hearty breakfast of porridge and milk. His reason was only now, however, found to be quite restored; for when awake at the former times, he still talked incoherently. Early this morning he had the last of the 6 grains of antimony ordered on my first visit. Quiet, and nourishing diet prescribed.

4*th day*.—Found him in all respects perfectly well—better than he had been for many months past, both physically and mentally, and now reading in bed and conversing intelligently.

Remarks.—A characteristic example of delirium tremens of average severity; occurring without any abstraction of the usual stimulants, even on the second day of the paroxysm; and yielding satisfactorily in almost 24 hours to the influence of antimony, aided by good nourishment and careful watching.

Case IX.—Formerly a butler, now a first-class lodging-house keeper, æt. 53. Florid complexion. Has drunk spirits for many years in a systematic way, beginning early in the morning; but confesses to much greater indulgence during several weeks past, and more especially two days before he came under treatment, when, as expressed by himself, he had been "much the worse of it" (intoxicated); and it appeared also that on this occasion he had partaken

largely of salmon, stewed rhubarb, and sundry other articles, which had disordered his stomach. At 5.30 next morning, he was seized with severe pain in the bowels, and soon thereafter with vomiting and purging. In the course of that and the following day, he had taken repeated doses of tincture of rhubarb, laudanum, and brandy, but without benefit, and I was sent for in the afternoon. I saw him at 7 *evening*. He was then much pained in the bowels and purged ; tongue exceedingly white ; thirst considerable ; urine not passed for many hours ; pulse 90, rather full and vibrating ; and extremities cold. Fearing an attack of cholera, I ordered a large sinapism to be applied to the epigastrium, heat to the feet, 5vj. of castor oil to be given immediately, and an opium pill, 1 grain, after the first movement of the bowels. Three other opium pills of the same strength were prescribed, one to be given at intervals of from two to four hours, according as the diarrhœa should be more or less urgent.

2*d day*, 11 A.M.—Has had 4 grains of opium during the night, which has checked the diarrhœa ; but he has slept none, and complains much of pain in the bowels, about which he is nervously anxious, fearing that some dangerous malady is in progress. His manner is considerably excited, and he is very restless. Pulse 100 ; pupils contracted. Another sinapism ordered to the epigastrium. I now perceived that an attack of delirium tremens was approaching, and regretted much that I had prescribed opium so freely.

6 P.M.—Pulse 116 ; tongue white ; perspiring copiously ; pupils still contracted ; no return of diarrhœa, and does not complain of abdominal pain ; hands slightly tremulous ; considerable agitation of manner ; voice weak ; speech rapid and stuttering ; and since the forenoon he has at times been talking incoherently, fancying that he saw strangers in the room, etc. To have ipecacuan wine ℥ xx every two hours, and some tea, or weak coffee and milk and bread for diet.

10 P.M.—Pulse 120. Symptoms of a paroxysm of delirium tremens confirmed. Is now out of bed, dressed, cheerful and active, as if there had been no previous ailment. Says that he is quite well, but that a number of "queer customers" have been visiting him, etc. To be strictly and judiciously watched, and the ipecacuan to be continued.

3*d day*, 11 A.M.—Pulse 108, feeble ; pupils quite natural, but eyes dull and expressionless. Urine dark and turbid. Has not been in bed at all during the night, but busy arranging furniture and hunting rats, particularly two old fellows, which, he avers, are hiding among the bed-clothes. Continue the ipecacuan and give beef-tea occasionally.

5 P.M.—Pulse 116, rather firmer than in the morning. Was permitted to walk out for an hour in the afternoon, well attended. He is now much more excited, but not quite so tremulous. Is perspiring copiously, and very earnestly catching imaginary objects in the air, which, however, are no sooner secured than they invariably slip through his fingers, and are broken and lost, thus occasioning him great distress. Ipecac. etc. to be continued. A specimen of urine passed in the morning became turbid with heat, and on evaporation presented a considerable abundance of phosphates.

10 P.M.—Head bathed in perspiration ; hands cold ; pupils natural ; pulse 112, and small. Urine high-coloured. Bowels not moved since early yesterday

morning. Is at present standing close to a wardrobe, where he says he has locked up in a small crevice "a female thief of the long-nebbed kind." To continue the ipecacuan, and to have a laxative dose of the Pulv. Rhei. Comp. to-morrow morning.

4th day, 9.50 A.M.—Perspiring much. Pulse 116, very weak, yet he is extremely active. Has not been in bed during the night, but going about very excited, and troubled with phantasms. Thinks he has been sentenced to be flogged on the Castle Hill in the forenoon, to the extent of fifty lashes; and is now shouting into the corner of a press "how many are on my side?" on the supposition apparently that a petition is to be got up to prevent the punishment from being carried into execution. Has had beef-tea at one time, and *café au lait* at another time this morning. The urine examined after this visit was clear, but high coloured, became slightly turbid with heat, and was found to contain numerous phosphatic stellar and penniform crystals.

5 P.M.—Pulse 120, weak; more tremulousness of hands than formerly, and some tendinous jerkings; and is more excited also than he has yet been. Every article of furniture in the room, even two very heavy wardrobes which might have been supposed quite beyond his strength, have been moved out of their places again and again; and he is now in the midst of his confusion, haggard-like, and bathed in perspiration, searching for seven children, who, he says, were sent from Newcastle. Ipecacuan to be continued, and nourishment as formerly.

11 P.M.—Has been sleeping quietly and calmly for the last hour, quite worn out apparently; by his exertions among the furniture, searching for thieves, dogs, and children; stopping up water pipes which had burst; replacing bell-ropes which had been pulled down, etc. During the afternoon he has had his shirt changed three times on account of the excessive perspiration. Had some minced collops in the afternoon, and some tea and bread in the evening. He has now had, within the last forty-eight hours, ℥i. ℥vj. of the ipecacuan wine. Ordered to be kept very quiet, so as to prolong the sleep if possible; and when he awakes, to have some ipecacuan and some strong beef-tea.

5th day, 11 A.M.—Last night had slept three hours, and then three hours again this morning; but between these periods he was as excited as formerly, and is now toiling as hard as ever among the furniture, which is piled up in the middle of the floor. He is covered with perspiration; looks very anxious and alarmed, and is very cross, contradicting whatever is said by his attendants. Pulse 120, and small; pupils somewhat dilated; urine still high coloured, but not so scanty, and only very slightly turbid with heat. To discontinue the ipecacuan, and to have instead, ℞. Tart. Antim. gr. iv., Iuf. Quassiæ ℥iv., a tablespoonful every two hours; nourishment also as formerly.

11 P.M.—Worn out by exertion; he had voluntarily gone to bed about 4 P.M., when he fell at once fast asleep, and has not awoke since. He is now breathing calmly, and his pulse is 96, soft, and not so weak as in the forenoon. Has only had one grain of the antimony, which is now to be discontinued.

6th day, 10.30 A.M.—Has slept well all night, awakening for a short time about midnight, when he got some nourishment, and then slept again until lately. Pulse 74. Mind quite clear and composed, and hands steady. Feels the whole body stiff and aching from the fatigue which he has undergone.

7th day, 11 A.M.—Continues improving. Feels more clearness of head and general lightness of the system than he has experienced for a long time past. Is to be up in the afternoon.

8th day.—Was able to walk out, quite well, and in every respect capable of resuming his ordinary occupations.

Remarks.—The above is a very severe instance of *delirium tremens* —the paroxysm lasting about seventy-seven hours. It was a first attack, and resulted from habitual drinking and no diminution of supplies; but on the contrary, excessive indulgence up to the period when seized with the choleraic symptoms—which probably precipitated or hastened on the disease, aided no doubt by the doses of brandy, laudanum, and tincture of rhubarb, taken before my first visit, and by the four grains of opium afterwards unfortunately prescribed to check the diarrhœa. In regard to the phenomena presented by this case, and the effect of remedial means, I would remark 1st, That the contracted state of the pupil only lasted while the effect of the opium continued. 2d, That the tendency to perspiration usually attending this affection was much increased apparently by the ipecacuanha, but without proving injurious. 3d, That the urine was found during the paroxysms, to be albuminous, and to present phosphatic crystals. 4th, That the speedy improvement following the change to the antimonial treatment suggests the probability that the attack would have been shorter, had that agent been employed earlier, but which was withheld from the supposed risk of bringing back the diarrhœa. 5th, That very considerable weakness of pulse may exist along with astonishing capability for muscular effort. 6th, That muscular exertion may be permitted with safety, nay, even with benefit, as a means of inducing natural sleep; and 7th, That this mode of treating the disease does not lead to any subsequent debility, but on the contrary, holds out the best expectation of recovery, with a sound condition of the mental and physical powers.

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ON THE

RELATIVE INFLUENCE

OF THE

MALE AND FEMALE PARENTS

IN THE

REPRODUCTION OF THE ANIMAL SPECIES.

BY

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THE RELATIVE INFLUENCE

OF THE

MALE AND FEMALE IN REPRODUCTION.

[READ BEFORE THE MEDICAL SOCIETY OF SOUTHAMPTON, JUNE 6, 1854.]

At a meeting of the Newcastle-upon-Tyne Farmer's Club, held on the 4th March last, Sir Matthew White Ridley, Bart., in the chair, Mr Orton of Sunderland read a paper on the "Physiology of Breeding," which excited great interest among the members present, and has since been published in the *Newcastle Chronicle* (March 10th).

The views therein set forth, supported as they are by a large body of evidence, seem well deserving the attention of the physiologist. Moreover, as they have important practical bearings in relation as well to our own species as to the lower animals, they possess a general interest, and concern not merely the physiologist and the farmer, but the physician also, the vital statist, the medical jurist, and others.

Mr Orton's argument is, that in the reproduction of the animal species there is no casual blending of the parts and qualities of the two parents, but that each parent contributes to the formation of certain structures and to the development of certain qualities. And advancing a step further, he maintains that the male parent chiefly determines the external characters, the general appearance, in fact, the outward structures and locomotive powers of the offspring (*e.g.*, the brain, nerves, organs of sense, and skin, and likewise the bones and muscles more particularly of the limbs), while the female parent chiefly determines the internal structures and the general size and quality, mainly furnishing the vital organs (*e.g.*, the heart, lungs, glands, and digestive organs), and giving tone and character to the vital functions of growth, nutrition, and secretion.

Not, however, that the male is wholly without influence on the internal organs and vital functions, or the female wholly without influence on the external organs and locomotive powers of their offspring. The law holds only "within certain restrictions." These may be said to constitute a secondary law—the law of limitations, equally important to be known as the fundamental law itself, and to be hereafter considered.

Such, according to Mr Orton, is a general fact or law of Nature. Claiming the merit of having worked it out by independent observations of his own, Mr Orton, nevertheless, gives the credit of its discovery to the late Mr Walker, and refers to this author's work on *Intermarriage*, as embodying views which, with certain qualifications, are identical with his own. It is scarcely necessary to remark, that this circumstance—of two independent observers in the same field of inquiry arriving unknown to each other at the same point—imparts to their common conclusion a strong *à priori* probability.

In proof and illustration of his general proposition, Mr Orton adduces a large collection of what appear to be strikingly apposite examples, taken from three several divisions of the animal kingdom, mammals, birds, and fishes.

Premising, what will afterwards more fully appear, that crosses or hybrids furnish the most remarkable examples of the proposition, and serve best to test it, we will now endeavour fairly to exhibit the kind and value of the evidence brought forward in support of it.

The *mule* is the produce of the male ass and the mare; the *hinny* (or as it is also called the *mute*), that of the horse and the she ass. Both hybrids are the progeny of the same set of animals. They differ widely, however, in their respective characters—the mule in all that relates to its external characters having the distinctive features of the ass,—the hinny, in the same respects, having all the distinctive features of the horse; while, in all that relates to the internal organs and vital qualities, the mule partakes of the characters of the horse, and the hinny of those of the ass.

"The mule, the produce of the male ass and mare, is essentially a *modified ass:* the ears are those of an ass somewhat shortened; the mane is that of the ass, erect; the tail is that of an ass; the skin and colour are those of an ass somewhat modified; the legs are slender and the hoofs high, narrow, and contracted like those of an ass. In fact, in all these respects it is an ass somewhat modified. The body and barrel (however) of the mule are round and full, in which it differs from the ass and resembles the mare.

"The hinny (or mute), on the other hand, the produce of the stallion and she ass, is essentially a *modified horse:* the ears are those of a horse somewhat lengthened; the mane flowing; the tail is bushy like that of the horse; the skin is finer like that of the horse, and the colour varies also like the horse; the legs are stronger, and the hoofs broad and expanded, like those of the horse. In fact, in all these respects, it is a horse somewhat modified. The body and barrel (however) of the hinny are flat and narrow, in which it differs from the horse and resembles its mother the ass."

In connection with these examples, Mr Orton refers to a special feature seen equally in the two instances, and which seems at first sight a departure from the principle laid down by him. It is this: Both hybrids, the mule and the hinny, take after the male parents in all their external characters, save one, which is *size*. In this respect they both follow the female parents, the mule being in all respects a larger and finer animal than its sire the ass; the hinny in all respects a smaller and inferior animal to its sire the horse; the

body and barrel of the mule being large and round; the body and barrel of the hinny flat and narrow; both animals being in these particulars the reverse of their respective sires, but both resembling their female parents.

In explanation of this seeming exception, Mr Orton adduces a well-known principle in physiology. The principle is, that the whole osseous framework is moulded in adaptation to the softer structures immediately related to it, to the muscles covering it in the case of the limbs, and to the viscera in that of the great cavities which it assists in forming. Accordingly, in perfect harmony with Mr Orton's views, the *general* size and form which must be mainly that of the *trunk*, will be determined by the size and character of the viscera of the chest and abdomen, and will therefore accord with that of the female parents by whom the viscera in question are chiefly furnished.

On the important but difficult subject of the restrictions within which the general law holds, or what may be called the *law of limitations*, Mr Orton makes some judicious observations :—

"I do not mean it to be inferred" he says, "that either parent gives either set of organs uninfluenced by the other parent; but merely that the leading characteristics and qualities of both sets of organs are due to the male on the one side and the female on the other, the opposite parent modifying them only. Thus, I do not infer that the ass has alone been the agent in conferring the external characteristics of the mule, but merely that he has principally conferred the developments, while the mare has been in regard to the external organs a secondary agent, an instrument not of conferring, but only of modifying those organs. It is just the reverse, however, with the vital organs. The female is the agent in conferring them, the male only an agent in modifying them. Hence I conceive that though the male and female parents in all cases give—the former the external, and the latter the internal organs, yet they each mutually exercise an influence in modifying to a greater or lesser extent the organs given by the other."

The instances given are in fact a proof of this. The mule, though essentially a modified ass, and the hinny, though essentially a modified horse, are neither of them anything else. In neither hybrid have we the perfect head and limbs of the ass or the horse grafted as it were on the proper body and barrel of the horse or the ass. The animals are both of them *composite*, though so specifically distinct the one from the other, and taking each of them to such an extent after their respective sires as to warrant the distinctive appellations given them by Mr Orton.

It was formerly remarked that crosses or hybrids serve best both to test and illustrate Mr Orton's law. It will now appear that in the first instance the law in question can only be satisfactorily investigated by observations confined to animals of this sort. For, if even in crosses or hybrids there is more or less of a blending, in every part, of the organs and qualities of the two parents, it is plain that in animals the produce of parents of the same species and of the same breed or variety, this blending, although not necessarily

greater than in crosses, must yet be such as often effectually to obscure to the ordinary observer the distinctive features of either parent. It is owing, doubtless, to this, that there is such diversity of opinion even among practical men on the subject, as any one will find by looking into any of the many treatises on cattle. The law, however, once clearly established in the way suggested, and the conditions or limits within which it holds ascertained, may afterwards be more easily and confidently applied to all cases.

Besides the influence which either parent exerts on the parts or qualities furnished by the other, and which may be regarded as innate and essential, various other influences control and modify the general law. One of these, and that on which Mr Orton chiefly dwells is, the effect on the breeding powers of the female produced by the male that *first* had *fruitful* intercourse with her, as seen in the impress of that male on the progeny she may subsequently have by other males. Of this we have a notable instance in Lord Morton's Arabian mare, which, after having a hybrid by a Quagga, bore successively three foals, every one of which had distinct traces of the Quagga. Having formerly[1] given a large collection of cases

[1] "Monthly Journal of Medical Science" for October 1849, and September 1850; and pamphlet by the author "On a Remarkable Effect of Cross-breeding," 1851. As supplementary to the examples of this influence there given, the following additional examples of it are extracted from Mr Orton's paper.

Referring to Lord Morton's mare, and to the influence exerted on her reproductive powers by the Quagga, Mr Orton says :—

"I myself made an experiment in illustration of it. Having procured a white bull-and-terrier bitch, which had previously been breeding with coloured dogs, I caused her to breed with a bull-and-terrier as like herself as well could be, and also pure white, reasoning that if this male influence existed, then the bitch, although breeding with a white dog and herself white, should throw coloured puppies. In her first litter of nine, there was not one which was not more or less coloured. In her second also of nine, four were white and five coloured. In her third (also to the same dog) all were white. The bitch had evidently been influenced by the previous impregnation, and it was also evident that by continuation of the influence of the white dog, the *stain*, if we may so call it, was gradually obliterated. Her fourth litter were all pure white. It was my intention to have carried her back to the coloured dog, reasoning that she would then breed one or two litters of white dogs; but unfortunately she ceased to breed, although lined by a coloured dog. I regret this the more, as I shall be years in bringing another animal to the same condition for experiment.

"I have, however, tried the same with pigs, breeding from a white sow and an Essex boar, and with the same result. The white sow had three litters from the Essex boar, and was then served by a white boar; three out of seven of the litter were coloured.

"About two years since I engaged to take two pigs from a litter. The boar and sow were of the improved Essex breed, and had both carried premiums. I of course looked for good bred pigs from them; but on going to make my selection I found the largest and best of the litter with more or less white on the feet and fetlocks. I protested against the purity of the breed, but being assured of it, it occurred to me that this might be a case of influence from previous impregnation. I asked the question without stating my reason for

of this sort, and endeavoured to show that the influence thus exerted by the male on the reproductive powers of the female is a *constitutional* one, and that it is brought about by the *fœtus in utero* INOCULATING the mother's system with the constitutional qualities of its sire inherent in it by necessary derivation from him, it may suffice to observe under this head, that among the lower animals, by reason of the promiscuous intercourse that obtains, the agency of the cause in question in modifying Mr Orton's law must be frequently in operation, and a source of error in conducting observations as to that law.

it; and at once it was admitted that previously to the owner procuring his Essex boar, the sow had been breeding with a white boar.

"Mr Storey of Hartwarren, had a brown mare which for its first foal was put to what he called a 'large, long-legged, Cleveland horse.' The produce was a brown foal. The mare was next twice served by a *black, short-legged* horse, and had two foals to him, both of which were bay, and, in Mr Storey's own words, 'large, tall, long-legged, Cleveland-looking horses.'

"Having myself a mare which I prized, I determined on breeding from her. She was grey, and was put to Wizzard, a grey horse, the produce was a brown colt. She was next served by Sheffield, a bay horse; the produce was a grey filly. She was then served by Clinty, a grey horse, and the produce was a bay colt. It is curious to observe that though grey herself, and twice served by grey horses, she only once threw the grey, and, moreover, that her foals never were the colour of the horse by which it was got, but always of the one which preceded him.

"It is remarkable that though the knowledge of this curious influence has only lately dawned upon Europe, the Asiatic seems to have been in possession of it. In Abdel Kader's celebrated letter on the Arab horse, the principle is touched upon. He states that a mare having bred with the ass is no longer fit to breed from—why, he does not say, perhaps does not himself know, but we can now easily see the reason."

In two former papers in this Journal (*ut supra*) "On the Fœtus in Utero as Inoculating the Maternal with the Peculiarities of the Paternal Organism," the author suggested whether the male parent, if tainted with the syphilitic poison, might not, through the fœtus, contaminate the female. This suggestion seems to accord with the experience of both M. Ricord and Dr Tyler Smith. M. Ricord admits that a mother may give birth to a syphilitic child without herself becoming subject to the disease; but his experience goes to prove that in the case of a woman pregnant of a child whose blood is contaminated with syphilis acquired from the father, this child may, and often actually does, contaminate the mother's system.—(See Mr Acton's work on *Syphilis*, p. 632.) And at a recent meeting of the Royal Medical and Chirurgical Society, Dr Tyler Smith is reported to have expressed himself as follows:—"With regard to the mode in which women became affected with syphilitic uterine disorder, he (Dr T. Smith) believed the most common was that in which the fœtus was the medium of communication. It often happened that men, presenting perhaps at marriage syphilitic sore throat or eruptions, impregnated healthy women, the result being a tainted ovum, which infected the mother. In some of these cases, the woman presented secondary uterine symptoms during pregnancy; in others, not until after delivery. The mildest symptoms in the father might be followed by very severe symptoms in the mother. He had observed in such cases, that at *each pregnancy* a *fresh dose* of the syphilitic poison was imparted to the mother, unless in the meantime the husband had been the subject of anti-syphilitic treatment."—See *Association Medical Journal* for July 14, 1854.

The foregoing comprises most at least of the general statements bearing on the subject referred to by Mr Orton. We may now proceed to the other illustrations given by him in support of his theory :—

(1). "The mule and hinny," says Mr Orton, "have been selected and placed first, because they afford the most conclusive evidence, and are the most familiar. Equally conclusive, though perhaps less striking instances may be drawn from other sources. Thus, it has been observed that when the ancona or other sheep are allowed to breed with common ewes, the cross is not a medium between the two breeds, but that the offspring retains in a great measure the short and twisted legs of the sire.

"Buffon made a cross between the male goat and ewe ; the resulting hybrid in all the instances, which were many, were strongly characteristic of the male parent, more particularly so in the hair and length of leg. Curious enough the number of teats in some of the cases corresponded with those of the goat.

"A cross between the male wolf and a bitch illustrates the same law ; the offspring having a markedly wolfish aspect ; skin, colour, ears, and tail. On the other hand, a cross between the dog and female wolf afforded animals much more dog-like in aspect—slouched ears and even pied in colour. If you look to the descriptions and illustrations of these two hybrids, you will perceive at a glance that the doubt arises to the mind in the case of the first, 'What genus of *wolf* is this ?' whereas in the case of the second, 'What a curious *mongrel dog !*'

(2.) "Amongst *birds* we have the same results, and they afford the like illustrations to our subject. Those who have had much to do with pigeons must have perceived that a cross between a *carrier* cock and *dragoon* hen is always a fine bird, and very nearly equal to the carrier ; whereas, a cross between a dragoon cock and carrier hen results in nothing better than a dragoon. Precisely the same may be observed in the cross between the *tumbler* and *pouter*.

"It is curious to observe, that the proposition I make regarding male influence should not only have been observed, but distinctly stated in so many words. Mr Lloyd says, 'The *capercailli* occasionally breed with the *black grouse*, and the produce are in Sweden called *racklehanen*. These partake of the leading characters of both parents, but their size and colour greatly depend upon whether they have been produced between the capercailli cock and grey hen, or *vice versa*.' (Yarrel, p. 293.) The hybrid between the pheasant and grouse is a striking illustration, showing so clearly its male parent : in almost all respects it is a pheasant, only the tail slightly shortened. It may be observed too, that the feathered feet of the grouse have disappeared in the offspring (Ibid. p. 309). Another instance of the same cross is given (p. 311), in which the general characteristics are those of the pheasant ; and this would have been still more striking if the tail had not been spread—a liberty, I suspect, either of the artist or the stuffer of the specimen. The legs in this instance are slightly feathered. Another hybrid is given (p. 313), between the *ptarmigan* and the grouse. Although the precise parentage of the bird is not stated, I am perfectly satisfied that in this case the grouse has been the male parent, and the tail indicated this, being somewhat forked and divergent. In your museum there is an interesting specimen illustrating the same law—a hybrid between the pheasant and grey hen. In this case the produce is pheasant-like in aspect, tail like the pheasant, but somewhat spread, no appearance of forking of the tail.

(3.) "Even in the breeding of *fish* the same law has been observed. Sir Anthony Carlisle produced mule fish by impregnating the spawn of the *salmon* by means of the male *trout*. The result I give in his own words :—'These mules partook of the character of the trout more than of salmon. They had bright red spots on their sides, but the black colour was shaded downward in

bars like those of the perch. The tails were not forked like those of the salmon, as I have seen them in the Thames *skeggers* (from which I infer the male salmon in that case to have been the impregnators.') We thus see in the case of fish, as in that of animals, the male parent giving the external characteristics ; those produced by the male trout had not forked tails. The skeggers, on the other hand, produced by the male salmon had forked tails."

In the course of his paper, Mr Orton adduces some special evidence in support of that part of his argument which relates to the office of the female in the work of reproduction. Admitting, as he does, that on this side of the subject the evidence is not so certain or conclusive, he gives several instances bearing chiefly on the milking, and on the nutrient and fattening qualities of the offspring, which qualities, he alleges, pass chiefly on the side of the female parent. He dwells at some length on the history of the *Short-horns*, the origin of which he conceives furnishes evidence in this direction. He refers to Mr Bakewell's Dishley sheep, as deriving and maintaining their celebrity through the ewes. And he adduces an observation which, if well founded, is of great pathological importance, and of especial interest to members of the medical profession, as well as to the directors of associations for life assurance. It is this : that diseases of the vital organs (and it may be presumed, therefore, diseases primarily involving the vital functions of nutrition and secretion) are transmitted oftener, and in a more intense form and degree on the side of the mother than that of the father. And he adds, that in the matter of life assurance, he has long been in the habit of judging of the value of a life by the family history on the female side. In perfect accordance with this position, and if well founded in their degree confirmatory of it, two observations may be cited ; the *first*, that the daughters of a woman who has herself borne a large family, are often equally prolific as their mother ; a fact, if it be one, not without interest to those to whom an heir-male of their own body is an object ; the *second*, that the daughters of mothers that have borne twins oftener than other females give birth to twins.

In connection with this branch of his subject, Mr Orton draws a distinction between a part or organ, including its vital endowments, and the *quality* of the organ and its endowments. And, while maintaining that the "outer" structures are chiefly furnished by the male parent, he equally holds that the quality of these, as of all the organs, comes mainly of the female. By quality, he obviously means what the older physiologists included under the term *vis vitæ*, or what in ordinary language is called *stamina*. It is not, for example, the special endowments of the nervous and muscular systems, the powers of contractility and nervous agency, considered *per se*, which he says are given by the female, but the quality of these as now defined, and which may be either good or bad. This allegation he illustrates by a reference to the "Short-horns," and Mr Bakewell's breed of sheep. But it will perhaps be best understood by a reference to the horse. "The Arab," he says, "will let you have his

stallion; but his mare at no price. He cultivates *endurance* and *bottom*, and the female gives them. He does not know the law we are promulgating; but he acts as if he did, for experience has taught him. The English breeder, on the other hand, values the stallion. He cultivates *speed*, and he finds that the sire gives the locomotive organs; consequently his value, just the reverse of the Arab; his mare is easily got at, but his stallion is priceless."

This distinction, if a real one, properly comes within the law of limitations, formerly spoken of, and forms an additional article of it. It is plain that it will be an additional source of difficulty and embarrassment in conducting observations as to the main law, but a means also of explaining seeming anomalies or discrepancies. Declining to give an opinion regarding it, it may be remarked in passing, that while *talent* is notoriously often hereditary in the male line, it has often been observed of individuals that have risen to distinction among their fellow-men, either by their *power* of intellect or *force* of character, that they have owed their pre-eminence to their mother.

The foregoing is a tolerably full abstract, and it is hoped a fair representation of Mr Orton's views. It would have been desirable to compare them with Mr Walker's, and to conjoin a like abstract of the facts and arguments of the latter. This we are at present unable to do. Mr Orton's own paper, however, is so well fitted to direct attention to the subject, and to give a definite bearing to scientific inquiry into it, that we would fain believe that the account now given of that paper will be not unacceptable to the profession. And submitting for their consideration some further remarks on the general subject, we will only here observe, that whether Mr Walker and Mr Orton's theory of it be correct or not, the process which they have undertaken to explain, and every minute detail of it, must be regulated by fixed law, and that the discovery of this law, if at all possible, must be reached through such observations as those founded upon by Mr Orton.

Connected with the subject of Mr Orton's paper, there are some facts and considerations not referred to by him, but in entire harmony with his theory, and which may serve both to lend support to it, and to impart to it a deeper practical interest.

1. While the fœtus is developed from that part of the ovum called by physiologists the *germinal* membrane, this membrane itself consists of *two* layers, an outer and an inner, called respectively the *serous* and the *mucous*. Of these layers, each gives origin to a special set or system of organs; the outer (or serous) to the brain, nerves, organs of sense, and integuments, and likewise to the bones and muscles; the inner (or mucous) to the lungs, glands, digestive organs, etc.

That is to say, the outer layer gives rise to the whole set of organs concerned in the strictly *animal* functions, while the inner layer gives origin to those concerned in the strictly *vital* functions. It is

scarcely necessary to remark how the very keel and foundation (so to speak) of every animal is laid down by nature in a manner which very exactly tallies with Mr Orton's position; or how the observations of the pure physiologist and those of the mere naturalist coincide!

With regard to the heart and blood-vessels, they would appear either to be the joint production of the two layers or to originate in an intermediate layer subsequently developed and called the *vascular* layer. But, however this may be, it may be remarked that the whole vascular system stands in the same relation to the vital organs and their functions that the osseous does to the muscular and to the locomotive powers,—that is, is *subordinate* to them. The vessels are mere carriers of the nutrient fluid, the blood,—mere channels of irrigation to the vital organs; not taking any other or more direct share in the vital processes,—the activity and whole character of which are determined by their own organs and the specific endowments of these.

2. It is now clearly made out as a matter of fact, by the researches of Wagner, Bischoff, and others, that in the impregnation of the mature female ovum, the essential element of the male semen (a spermatozoon) is brought into actual *contact* with the ovum; and there can be no reasonable doubt that such contact is an indispensable condition in the process.

But there is another fact of even greater importance in relation to our present subject, for which we are indebted to the late Mr Newport. The spermatozoon is not simply brought into contact with the ovum, but into the closest possible *union* and *incorporation* with it. When that contact occurs, according to Mr Newport, the spermatozoon embraces the ovum (ovule) on every side, grasping it firmly, but quickly undergoes solution and disappears; being, in fact, either absorbed by or becoming intimately blended with some part of the ovum.

Further than this, we cannot at present go. We are still ignorant of the relation which, in this act of union, obtains between the spermatozoon and the two layers of the germinal membrane that are subsequently evolved. And as the spermatozoon so quickly disappears by solution, it seems impossible that we shall ever be able to determine as to this. But as the spermatozoon comes first into contact with the *exterior* of the ovum and disappears while lying there, it seems not unreasonable to suppose that its influence while extending to the outer layer may be in a great measure confined to it. And Mr Orton's law (if a real one) would go far to establish this supposition. That layer gives origin to the "outer" structures of the animal, *i.e.*, to the nervous, osseous, and muscular systems; while, according to Mr Orton, it is the male parent that chiefly furnishes these.

So much for the facts. The following considerations seem not

unconnected both with those now stated and those adduced by Mr Orton.

First, Of the several parts composing the animal body, and of the several functions performed by it, those called by physiologists the *animal* in contradistinction to the organic or vital, are all that can properly be regarded as essential to any animal and distinctive of it. The vital organs and their functions have no other objects in the economy than the preservation and maintenance of the animal organs; their growth in the first instance, and, contingent on the tear and wear which their exercise involves (and that in a very peculiar degree), their renewal during the whole period of life. But for this, there would be no need of the vital organs and their functions —which do not differ, it may be added, in their essential nature from those of vegetables.

In short, brain, nerves, and organs of sense, bones, and muscles,—the organs and instruments of a *sentient* being, and bestowed for the purpose of establishing the relations of this being to external objects, and of investing it with more or less of voluntary power over the surrounding world—are all that properly enter into our idea of an animal. And possibly in our future state of being, in which, as Scripture teaches, we shall hunger no more, nor thirst any more, in which there will be no decay, neither disease, nor death, and in which likewise there will be neither marrying nor giving in marriage, the "incorruptible" and "glorified" body, suitably modified doubtless in adaptation as well to the absence of parts no longer needed as to the enlarged capacities and powers of its immaterial inhabitant, may consist of nothing more.

Secondly, Keeping in view what has been already said as to the origin of the external structures in the outer layer of the germinal membrane, and likewise as to these being the structures which are specifically distinctive of the animal; it may next be observed, that of each species of animal, the *male* must be regarded as the proper representative. Our great progenitor, *Adam*, was not only formed first, but lived sometime—longer perhaps than we are wont to imagine, single and alone, the sole and only representative of our race. "The head of the woman is the man;" "neither was the man created for the woman, but the woman for the man." In the reproduction of the species, accordingly, it might *à priori* be expected that the male parent should impart of his own qualities precisely those which are characteristic as well of the animal in general as of the species to which he belongs; that is, his bones and muscles, his brain and nerves, and it may be added, his own mental peculiarities.

And that the female should furnish the vital organs and give tone and character to the vital functions,—the feeding and fattening powers of the offspring, its secreting powers (*e. g.* in respect of milk), and likewise its general quality in so far as influenced or determined by the nutrient powers, we may reasonably enough suppose. Such a notion would be in keeping with what is obviously the subordinate

character of these functions, subordinate not certainly in the eye of the breeder, but in relation to the order of nature in the animal economy. Moreover, it would harmonize with what is palpably the part assigned to the female in the rearing and tending of her offspring.

Thirdly, We read in Scripture of *Levi*, yet unbegotten, being "*in the loins*" of his father. We speak of a father being *reproduced* in his son. And it is the boast of some of our nobility and old families, that they have passed through a long course of ages, in the *direct male* line.

Formerly, when the whole influence exerted by the male in the act of reproduction was supposed to be nothing more than a mere "*vivifying*" of the female ovum; when the whole virtue of the seminal fluid was thought to be a mere "*aura*," the ideas in question had no proper value. The female was presumed to bear the chief part and the essential part in the production of the offspring. In the facts before us, however, those ideas have their full significancy given to them. The male not only takes a direct share in the production of his offspring, but actually contributes that part which constitutes his own proper self; stamping his offspring at the same time with his own proper image and superscription.

What gives special value to the notion of descent in the male line is, that though the daughter equally with the son represents their common father, her children do not. Her brother's children do. But her's must be looked upon as representing mainly her husband's house and line, whose name accordingly they bear. Yet in "leaving" her father's house and "joining" herself to another, while she is but following the order and appointment of nature, she has her reward. Not only are her interests identified with her husband's and her affections transferred to him, but she becomes "bone of his bone, and flesh of his flesh,"—the two being "no more twain but one flesh."

And what if these expressions have a deeper meaning than at first appears? What if they be physiologically as well as figuratively true? For, according to Mr M'Gillavray,[1] through the offspring, and by them while *in utero*, the woman comes to be *inoculated* with those of her husband's qualities that are inherent in them, and were imparted to them in the act of impregnation,—so inoculated, that is to say, with those qualities as to be able herself to impart them to offspring she may subsequently have by another husband; while, according to Mr Orton, the qualities which the husband thus imparts, are his locomotive, *i. e.* his *bones* and his *flesh*. And to carry this speculation only one step further,—what if, as unquestionably he can to his offspring, what if the husband can thus and through them impart to his better-self of his own *mental* attributes, and thus there be engendered a blending of heart and mind, in a sense altogether

[1] See papers by the author, in the "Monthly Journal of Medical Science" for Oct. 1849, and September 1850, "On the Fœtus in Utero, as inoculating the maternal with the peculiarities of the paternal organism."

speeial and in a way otherwise impossible? It is a eommon observation at least, that husband and wife eome to resemble eaeh other not only in faee and feature, but in sentiment and feeling.

As for these speculations, however, *valeant quantum valeant.* If they eannot be received as established truths, they may, peradventure, be taken as "guesses at truth;"—bearing in mind as we ever ought, that "there are more things in heaven and earth" than either our mieroseopes, or our balanees, or our re-agents can give aeeount of,—or even than is yet "dreamt of in our philosophy."

SOUTHAMPTON, July 1854.

APPENDIX.

WHILST this reprint is passing through the press, the author has reeeived, and is kindly permitted to publish, the following interesting eommunieation from Dr GEORGE WILSON, M.D., F.R.S.E, Leeturer on Chemistry, Edinburgh, and author of the "Life of Dr John Reid," and of the "Life and Works of the Hon. Henry Cavendish," ete. ete.

"Edinburgh, August 7, 1854.

"DEAR SIR,—I make no apology for troubling you with a note in referenee to the very eurious subjeet on whieh you have a paper in the last number of the *Monthly Medical Journal.* It has struek me that the following faets might be interesting to you and to Mr Orton, as well as my old pupil, Mr M'Gillavray.

"You are probably aware that there exists in the Isle of Man a breed of eats, now beeoming searee, eharaeterized by the absenee of a tail. In eompensation for this they have mueh longer hind-legs than ordinary eats, in aeeordanee with the law pointed out by Geoffroy St Hilaire, or some other of the foreign so-ealled transcendental anatomists, viz., that there is a balance or equipoise of organs in every species, so that if an aberrant variety wants one charaeteristic organ of the speeies from whieh it has strayed, some other organ or organs are over-developed to atone for this. In the 'Manx' eats the missing tail appears in the long hind-legs; and the general appearanee of the creature is like that of the hare or rabbit. They are never very large, but they are abundantly aetive, exeellent runners and leapers, and though far from handsome, as eats are eounted handsome, amusing eompanions, from their odd

motions and the singular postures they assume when at rest. For such inquiries as you are pursuing, they seem peculiarly suited, and I shall now mention some points connected with their breeding which may be of interest to you.

"Some years ago a friend brought me from Liverpool a female Manx cat. She was of small size, black, very gentle, sagacious, and docile, and soon became a great pet. I had her in my possession two or three years, during which time she had three litters of kittens. The male parents of these were doubtless cats of the ordinary breed; for, besides that there were no Manx tom-cats in my neighbourhood, all the kittens *had tails*. These were short, odd-looking appendages in most of the animals; in one the tail curled like a greyhound's, and in all it had the character of an imperfect organ, over which its possessor had little power. Some of the females of this semi-tailed breed grew up, and their kittens by tailed cats would not have been distinguished by an ordinary observer from those of the common cat.

"I regret that I did not pay more attention to the relative length of the limbs and other points in the configuration of those kittens. But I had no special motive for watching them, such as I should in similar circumstances feel now. However, the fact, you will perceive, was decisively ascertained, viz., that the female parent being tail-less, and the male parent tailed, the kittens all had tails, such as they were.

"Well; two years ago, I got a male Manx kitten, which has grown up to be a strong tom-cat, and is at present in my possession. He is red, rough-haired, a most successful mouser and bird-catcher, and when on his legs a very active though awkward looking fellow. His days are spent chiefly in sleeping; his nights in marauding through the neighbouring fields and gardens, and in gambolling or fighting with the cats about the adjoining houses.

"Two days ago I incidentally learned that a female cat in a garden adjoining mine, with which my cat had often been seen frisking, had recently had kittens, and on making inquiry regarding their appearance, I learned that the litter consisted of eight, *five* of which had no tails, and were in consequence (much to my annoyance) drowned incontinently; the other three are described as resembling their mother, who is a large, handsome, brindled cat, such as would be called a tortoiseshell if the colours (black and red) were mottled together, instead of being arranged in irregular stripes.

"I further learned that on a previous recent occasion, this cat had nine kittens, of which *eight* were tail-less; and on an earlier occasion six kittens, of which *four* were tail-less; so that altogether of twenty-three kittens, seventeen were without tails, and six had them. These have all been born since my cat came here about a year ago. Before his arrival their mother had had kittens, doubtless by a cat like herself, for they were fully equipped with tails. Unfortunately, my neighbours regarding cats without tails as very ugly

things, have uniformly drowned them without letting us know of their birth, so that I can give you far less information than I could wish on the matter.

"You will see, however, from the facts communicated, that where the Manx cat was the mother, the kittens had tails of a sort; where the Manx cat was the father, three-fourths of the kittens had no tails. I may add, that the mother of these is a larger and more powerful cat than the male, and has an ample tail. You will notice that she had kittens by a common cat, before having any by the Manx one; you will mark, also, with interest, that in her first litter by the latter there were two out of six kittens with tails, which, I suppose you, and Mr Orton, and Mr M'Gillavray, would be inclined to refer to the influence of the previous male parent; and this conclusion would be borne out by the condition of the second litter by the Manx cat, where of nine kittens only one had a tail: but in the third litter of eight, three had tails. It is, of course, quite possible that the female had intercourse with other cats than mine.

"Our neighbours have kept one of the tailed kittens of the last litter, and I shall examine it when fuller grown as to its resemblance to its parents. If you will indicate any points as of special importance I will look to them.[1] It has been promised also that if there be another litter of tail-less cats, some of them shall be kept.

"I should mention that there are, or were, Manx cats in our Zoological Gardens here. By writing Dr Dumbreck you might learn about their breeding something interesting.

"I got my last Manx cat from a clergyman near Montrose, who has a great assortment of cats. If you would like to communicate with him, I shall endeavour to procure his exact address.

"I remain, yours truly,

"GEORGE WILSON.

"ALEXANDER HARVEY, Esq., M.D."

[1] The main points may be said to be, 1. Colour; 2. Size and conformation, particularly in respect of the tail and hind-legs; and, 3. Mental peculiarities and habits.—A. H.

DR CARNOCHAN'S CASE OF EXSECTION OF THE ENTIRE ULNA.

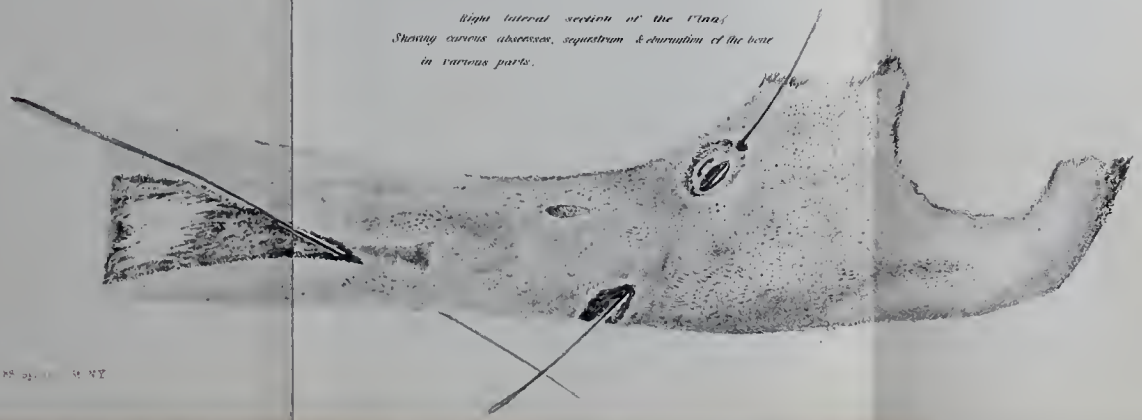
DISEASED RIGHT ULNA,- EXACT SIZE,

FIG. 1.

Shews chronic enlarged oval & round foramina & acicular & mammillated formations, on the surface of the bone.

FIG. 2.

Right lateral section of the Ulna, Shewing carious abscesses, sequestrum & eburnation of the bone in various parts.



Currier's Lith. 88 [illegible] N.Y.

Flanket Delt

EXSECTION

OF THE

ENTIRE ULNA,

BY

J. M. CARNOCHAN, M. D.,

PROFESSOR OF SURGERY IN THE NEW YORK MEDICAL COLLEGE, CHIEF SURGEON TO THE STATE EMIGRANTS' HOSPITAL, ETC.,

WITH A PLATE.

[From the American Medical Monthly, March, 1854.]

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1854.

EXSECTION

OF THE

ENTIRE ULNA.

Modern surgery is chiefly indebted for the revival of the operation for the exsection, partial or complete, of the long bones, to Moreau, Percy, Champion, Pelletan, and Dupuytren. Large bones of this class have been exsected in their contiguities: thus, Butts, of Virginia, exsected the entire radius; the fibula has been removed from one extremity to the other; and even the entire lower jaw has been at once removed, at both temporo-maxillary articulations, with satisfactory results. No instance, however, has been, as yet, recorded of exsection of the entire ulna. The following case shows that the entire ulna can be removed, and the functions of the upper extremity be retained, nearly in their original perfection.

Case.—P. Cavanagh, a native of Ireland, aged 30, of sanguineous temperament, of small stature, strumous aspect, without syphilitic taint, a shoemaker by trade, while splitting wood with a heavy axe, sprained his arm so severely that, as he expresses it, the sinews seemed to give way. During the night following the accident, he was awakened by intense pain about the region of the wrist joint. This was speedily succeeded by swelling of the upper and forearm, as high up as the humero-scapular articulation. In this condition, he consulted a physician, who prescribed an anodyne liniment, to be applied to the arm. The application was used for five weeks without abatement of the pain. Fomentations of hop leaves were then resorted to. These failing to bring relief, and the malady still progressing, the patient sought the advice of Dr. Webster, of Geneva, who made along the arm two deep incisions, which were followed by a slight discharge of pus and much blood. Cataplasms were then used for about eight weeks, with no relief to the pain or diminution of the tumefaction. In the month of July, 1852, Cavanagh entered as a patient the surgical division of the State Emigrants' Hospital.

At the time of his entrance he was much enfeebled and emaciated; the presence of irritative fever showed that the constitution was sympathizing

with the local disease; the hand, forearm, arm, and shoulder presented one dense, hard, tumefied, and shapeless mass, of a purple hue, and extremely sensitive when handled: the pain was unremitting, being more severe by night than by day; the circumference of the diseased forearm was three times greater than that of the corresponding portion of the healthy arm; and the density of the tissues was such, that, in connection with the wan and emaciated aspect of the patient and the purple hue of the integuments, there was reason to conjecture that the disease was one of malignant character. A lotion of acetate of lead and tincture of opium was ordered to be kept on the arm, which was also to be enveloped in oil-silk. Quinia, porter, and good diet were likewise ordered.

August 1st, 1852. Three weeks having elapsed, and the tension and swelling still remaining unabated, free and deep incisions were made through the tissues of the forearm; but the relief obtained by this operation was but momentary. The arm was now kept enveloped in a flax-seed cataplasm, with which was incorporated some extract of stramonium. During the months of September and October, the constitutional treatment was but little varied. Iod. ferri, and iod. potassii were at times substituted for the quinia; an anodyne draught of morphia was regularly given at bed-time. The topical applications consisted alternately of cataplasms, anodyne liniments, anodyne fomentations, of *eau sedative*, of extract of stramonium. While this treatment gave no relief to the pain, several abscesses had formed along the ulnar region of the forearm, and these openings left sinuses leading to the surface of the ulna; which, by means of the probe, could be felt, denuded of its periosteum.

The diagnosis now became more precise, and his card was ordered to be marked "Ostitis, caries and necrosis of the ulna, possibly, also, of the radius." The general tumefaction, at this period of the disease, rendered it impossible to ascertain that one bone alone was affected. The patient deriving no benefit from the use of the various medicamental means which had been resorted to, was recommended to remove from the Hospital to the country, for a change of air, and, at the same time, was directed to use tonic remedies and a generous diet. The patient consequently took his discharge from the Hospital, Dec. 1st, 1852.

On the 18th May, 1853, he was again admitted, having, in the interval, followed the instructions he had received. The shoulder and upper arm were now found to have resumed their normal appearance and size; but the elbow joint was very much enlarged, and almost incapable of motion. The forearm was still dense and hard, and was, moreover, much increased in size, presenting along its ulnar aspect a purplish hue, with various openings and sinuses, from which, at times, small portions of dead bone had been eliminated during the patient's absence from the Hospital. The wrist joint

was also limited in its movements, and supination and pronation could not be performed. The general health was somewhat improved, but the constitution still showed signs of participation in the local malady, and a dull and aching pain continued to extend along the arm towards the axilla.

The indications of treatment, now, were to keep up and improve the general tone of the system, and to use topically anodyne applications, in conjunction with iodurettèd preparations. To this end, during the following seven months, the constitution was supported by the internal exhibition of quinia, carb. ferri. precip., iodide of potassium, syr. iodide of iron, sarsaparilla, infusion of prunus Virginiana, wine, porter, generous diet, &c.; while locally, anodyne and iodurettèd cataplasms, fomentations, unguents, and the warm bath, were sedulously employed. But, from this treatment, no perceptible amelioration was obtained; the arm was still much tumefied and hard; the sinuses remained unclosed, discharging daily considerable quantities of purulent material, in which, at times, were found minute portions of diseased bone. At this time, also, Jan. 1st, 1854, the ulna could be more distinctly traced, and felt to be enormously enlarged, apparently through its whole extent; but there was good reason to infer, as no sinus could be traced to the surface of the *radius*, that this latter bone was entirely sound.

Medicamental and dietetic treatment had now been used for nearly two years; the arm was still useless, and a painful incumbrance; and the ultimate cure of the malady appeared to be beyond the resources of the medicinal art. The patient was becoming impatient, and anxious to obtain relief. The resources of operative surgery seemed now to offer the only prospect of attaining a serviceable result; and, as a point of practice, the alternative presented itself of amputation of the arm above the elbow, or of exsection of the entire diseased bone. From some recent investigations which I had been prosecuting upon the lower animals, I had convinced myself that the entire ulna, although forming an important part of the elbow joint, could be removed without materially impairing either the strength of the limb or freedom of its movements. Accordingly, I gave the preference to exsection of the bone, rather than to the severe mutilation of amputation of the arm, and performed the operation on the 14th January last.

Operation.—The patient was brought into the amphitheatre, and placed supine upon the operating-table. The assistants were arranged so as to maintain firmly the trunk and lower extremities, and be in readiness to hand the instruments and to sponge the wound. Chloroform was cautiously administered. While under the full influence of the anæsthetic, the position of the patient was changed so that he lay partly on the left side.

One assistant held and supported the upper arm of the diseased limb, compressing at the same time the humeral artery; another, seizing the hand

and wrist, rotated inwards the limb from the shoulder-joint, and carried the pronation of the forearm so far as to cause the palm of the hand to look directly outwards. The elbow-joint was now slightly flexed, and the hand elevated. This twisted position of the ulna upon the radius placed the ulna upon the posterior and outer aspect of the forearm, and rendered it more easily accessible.

The limb thus placed, the assistants maintaining the arm and forearm steadily, standing upon the right side of the patient and placing the fingers of the left hand upon the integuments of the forearm towards the elbow, with a strong, straight, sharp-pointed bistoury, I made an incision along the posterior and inner aspect of the ulna, commencing at the lower part of its superior third and extending downwards to a point over the extremity of the styloid process. This divided the tegumentary layers and fascia, which were found dense, matted, and infiltrated. The tendon of the *extensor carpi ulnaris* was pulled back, and the bone exposed. This was found rough, enormously enlarged, and presenting numerous oval foramina and several cloacæ, which communicated externally through the integuments. It was now apparent that the bone must be disarticulated. To effect this at the carpo-ulnar articulation, a transverse incision, about an inch long, parting from the lower extremity of the first incision, was made across the back of the wrist. The superficial tissues were here reflected, and the tendon of the *extensor carpi ulnaris* was carefully detached from its groove on the lower part of the ulna. The dissection was now carried along the anterior surface of the lower portion of the ulna, and the soft parts were detached from the bone as far as the interosseous ligament, the ulnar artery and nerve being carefully avoided. The soft parts were now detached from the posterior surface of the ulna, avoiding injury to the extensor tendons. An attempt was then made to pass a chain-saw around the ulna through the interosseous space opposite the lower part of the middle third. This was found impossible, on account of the approximation of the enlarged ulna to the radius, and the almost complete obliteration of the interosseous space. To divide the bone at this point, a small convex-edged saw was used. The bone thus divided, the interosseous ligament was detached downwards, and the lower fragment of the ulna was disarticulated from its inferior attachments to the radius, fibro-cartilage and the carpus.

It now remained to isolate and detach the upper fragment. The first incision was now prolonged upwards along the posterior surface of the ulna, so as to end at the upper part of the olecranon, opposite its outer edge. To this a terminal incision was joined, which extended transversely across the back of the elbow-joint as far as the inner margin of the ulna. The soft tissues were now dissected from the bone upon its posterior and anterior aspects, as far as the interosseous ligament and as high up as the insertion

of the *brachialis internus* muscle. The bone was next seized and pulled from the radius, and a knife, curved flatwise, was passed close upon its interosseal margin, and grazing the bone, the interosseal membrane was divided upwards, the soft parts being held apart, and the interosseal and ulnar arteries proteeted.

The elbow-joint was now flexed, and opened behind by entering the bistoury close to the inner edge of the olecranon, and the attachment of the trieeps extensor was next divided by cutting directly outwards. The ulnar nerve was now found, and hooked aside until farther dissection of the soft tissues was effected from the inner aspect of the joint and the upper part of the bone. The lateral ligament was next divided. The bone still remained firmly attached, chiefly by the coronary ligament and the insertion of the *brachialis anticus.* The ulna was earried backwards so as to make this muscle tense, and by carefully grazing the coronoid process with the knife the tendon was detaehed. Some diffieulty was here presented in avoiding the humeral artery, which lay in elose proximity to the enlarged coronoid process. The bistoury was now passed between the ulna and radius, and the coronary ligament divided. A few remaining fibres were divided, and the bone was eompletely detached.

During the operation there was a eonsiderable flow of venous hemorrhage, which soon eeased upon removal of compression from the upper arm. The arterial bleeding was arrested by torsion of a few arteries around the elbow-joint. The operation was performed in the presence of many pupils and professional gentlemen; and I was ably assisted during its different steps by Dr. Glück and Dr. Melville, of this city, and by Drs. Hensley, Gould, Harris, and Thomas, the Resident Assistant Surgical Staff of the Hospital.

Progress of Union.—After the operation, the wound was eleansed of coagula, and the edges brought together by ten points of interrupted suture. The limb, after the dressing and bandage were applied, was placed, prone and slightly flexed, upon a well padded splint, and fixed to this by eireular strips of bandage. The patient recovered slowly from the influence of the ehloroform, the pulse remaining below 50 for some hours; anodyne ordered at bed-time. Next day, Jan. 15th, the pulse 100—full and regular; oozing of blood has occurred to some extent; during the night, patient has been restless, and has suffered much pain in the arm. Sol. sulph. morph. at bed-time.

Jan. 16th. Pulse 100—not so full or strong; no more oozing of blood has occurred, and the patient feels more comfortable, having slept, and suffered but little during the night. The first dressing removed in the afternoon: for four inches above the wrist joint, the wound seems to be uniting by first intention.

Jan. 17th. Pulse 83—regular; general condition good. Ol. ricini ordered. The wound dressed; suppuration profuse. The lips of the wound have an unhealthy aspect; four of the sutures come away. Anodyne in the evening. The patient is ordered to commence in the morning with solution of sulphate of quinia.

Jan. 18th. Patient has slept badly, having suffered much pain, during the night, along the arm; pulse 80. Dressed the wound, which has assumed a better appearance; suppuration less, but little adhesion. Beef-tea ordered.

Jan 19. Pulse 90; patient has slept tolerably well. Wound dressed; discharge of pus decreasing, and union progressing from the wrist upwards; free discharge of synovial fluid from the elbow joint, upon removal of the dressing.

Jan. 20th. Pulse 84. Wound dressed; favorable progress. Full diet allowed. Quinine continued. No undue inflammatory action at either articulation. Arm still kept in the same position.

21st. Patient has suffered much pain at the elbow joint during the night. In the afternoon, wound dressed; doing well; there is free motion at both elbow and wrist joints; discharge of synovia still coming from the elbow joint.

Jan. 22d. Everything going on well. Wound dressed; but little discharge, except at the several tegumentary orifices which existed between the wrist and elbow before the removal of the bone; but little synovial fluid coming from the joint.

Jan. 25th. General condition of the patient excellent; pulse 80, and natural. Appetite good. Only slight oozing of synovia from the elbow; no pain. Splint upon which the arm rested in a state of pronation, dispensed with; forearm now bent at a right angle, and held in a position between supination and pronation, while a light, well-padded splint, extending from the elbow to the extremity of the fingers, is placed and bandaged along the front thereof, to support the radius; limb, thus adjusted, supported by a sling passed around the neck. Patient allowed to sit up.

Jan. 29th. First splint removed, and the arm, which had been maintained fixed for the last four days, adjusted, and bandaged to another splint, jointed and formed of two pieces, one for the upper arm, and another for the forearm; the joint being opposite the elbow, in front: by this arrangement the forearm still kept in semi-pronation, and radius supported, while, by regulating the angle of the splint, by a mechanism for that purpose, the forearm can be gently and gradually extended.

Feb. 5th. During the use of both splints, dressings carefully attended to, by removal and re-adjustment at suitable intervals. To-day, upon removal of the splints and dressing, healing process of the wound found to be

entirely completed; the tissues about the wrists and elbow joints being entirely consolidated, and free motion at both articulations possible by the patient himself, without any assistanee.

Feb. 10th. Limb still supported by a light bandage, and by the last splint, for the purpose of allowing the tissues along the line of the inner aspeet of the forearm to become further eonsolidated. Health of the patient is now good; he walks about like a well person. He is still upon tonic treatment, and is allowed generous diet.

Feb. 15th. Removed the splint; patient allowed to use his arm. General health entirely restored.

Feb. 18th. Five weeks after the operation, diseharged from the Hospital eured.

Appearance of the Arm; and its Functions.—With the exeeption of a depression, and the cicatrix along the ulnar aspect of the forearm, there is no deformity of the limb.

The functions of the arm are preserved in a remarkable degree of perfeetion. The power of prehension is unimpaired; and flexion and extension at both the elbow joint and at the wrist joint can be performed with faeility —supination and pronation can also be effected—abduction and adduction at the wrist joint ean be performed; as also flexion and extension of the fingers, as before the operation; sensation and nutrition are as perfect as on the arm and hand of the opposite side.

None of the large nerves or arteries were injured during the exsection of the bone, and the museular tissue was earefully preserved from the aetion of the bistoury, with the exeeption of the eubital origin and insertion of those muscles which are attaehed to the upper portion of the ulna. These had to be divided during the detaehment of this portion of the ulna.

Flexion at the elbow joint is chiefly effected by the *biceps flexor*, which is inserted into the tubercle of the radius; but the humeral origin of the other flexor muscles—sueh as the *flexor sublimis digitorum communis*, the *flexor carpi ulnaris*, the *palmaris longus*, the *flexor carpi radialis*, and the *pronator radii teres*—remaining uninjured, they also serve as auxiliaries in this function.

The *triceps extensor* and *anconæus* were, necessarily, entirely detached during the operation; but extension of the forearm is sufficiently performed by the action of the *extensores carpi radialis longus et brevis;* by the *extensor communis digitorum*, the *extensor minimi digiti*, and by the *extensor carpi ulnaris;* all of which muscles pass from the external condyle of the humerus, to be inserted on the posterior surface of different metacarpal and phalangeal bones of the hand.

Flexion of the wrist joint is effected by *flexor carpi radialis*, *palmaris*

longus, *flexor carpi ulnaris ;* extension, by the *extensores carpi radialis* and the *extensor carpi ulnaris.* Adduction, also, is effected by the *extensor carpi ulnaris ;* while abduction results from the action of the *extensores carpi radialis.*

Flexion of the fingers is chiefly effected by the *flexor sublimis digitorum communis*, and the extending function of the phalanges results mainly from the action of the *extensor communis digitorum.*

Pathological condition of the bone.—The diseased ulna is delineated in the plate, Fig. 1, and presents all the characteristic manifestations of prolonged inflammatory action of a high grade. The bone is enormously expanded from one extremity to the other—at the base of the coronoid process it measures in circumference 5½ inches; and its weight is 8 oz., minus 20 grs., the weight of a recent, healthy, adult ulna varying from 2½ to 3 oz.

Bony vegetations have assumed the acicular form on the radial aspect of the bone, on a line with the attachment of the interosseous ligament, as far down as the junction of the middle with the lower third—the acicular formations also prevail on and below the coronoid process. At all other points around the upper extremity of the bone, irregular mammillated appearances exist, with innumerable enlarged, round, and oval foramina. These enlarged foramina, in conjunction with the hypertrophied condition of the bone, are characteristic signs of protracted inflammatory action, as was long ago demonstrated by the Goodsirs, of Edinburgh.

Along the inner and posterior aspect of the bone exist some eight *cloacæ*, five of which are in the upper third of the bone: two in the middle third; and one near the styloid process. One of these *cloacæ*, situated between the coronoid and olecranon processes, communicates with the interior of the elbow joint; while another, situated at the lower part of the bone, communicates with the ulno-carpal articulation.

The other *cloacæ* pass deeply into the interior of the bone, ramifying extensively, like sinuses, in different directions along the inner texture; some of the sinuses containing portions of bone in a state of necrosis, and more or less detached. From these *cloacæ*, which opened externally upon the integumentary surface, large quantities of purulent fluid, mixed at times with portions of dead bone were discharged.

At the middle third, the circumference of the bone, by measurement, is four inches, being ⅝ in. larger than the shaft of an adult femur. At this part, also, the round and oval foramina are abundant.

The lower third of the bone is also extensively hypertrophied, being, at its upper part, 3⅜ inches, while, at the base of the styloid process, the circumference is 2⅝ inches.

The section of the bone, as represented in the plate, Fig. 2, shows the

appearance of the central portions. Here the influence of high inflammatory action, and its consequences, carious ulceration, necrosis, and eburnation, are plainly manifested. The greater part of the interior of the bone is exceedingly dense and compact. The surface of the osseous section is in some parts tinted of a dark purple hue; at other parts it is whitish and dense, like ivory, blastema having been here thrown out so as to obliterate the spongy structure, the Haversian canals, the lacunæ, and canaliculi. The right lateral half of the section [as seen in fig. 2] also shows the presence of two carious abscesses in the interior of the bone, which communicate externally with cloacæ and the integuments. In one of these abscesses a piece of sequestrum is situated, partly detached. Vide plate, Figs. 1 and 2.

20th Feb'y, 766 Broadway.

ADDRESS

DELIVERED AT

THE ANNIVERSARY MEETING

OF THE

GEOLOGICAL SOCIETY OF LONDON,

On the 17*th of* FEBRUARY, 1854;

PREFACED BY

THE ANNOUNCEMENT OF THE AWARD

OF THE

WOLLASTON PALLADIUM-MEDAL

AND PROCEEDS OF THE DONATION FUND

FOR THE SAME YEAR.

BY EDWARD FORBES, ESQ.,

PRESIDENT OF THE SOCIETY.

LONDON:

PRINTED BY TAYLOR AND FRANCIS,

RED LION COURT, FLEET STREET.

1854.

PROCEEDINGS

AT THE

ANNUAL GENERAL MEETING,

17TH FEBRUARY, 1854.

Award of the Wollaston Medal and Donation Fund.

After the Reports of the Council had been read, the President, Prof. E. Forbes, on delivering to R. Griffith, Esq., LL.D., F.G.S., the Wollaston Palladium Medal, addressed him as follows:—

Dr. Griffith,—It is my privilege, and one that I value most highly, to deliver to you the Wollaston Palladium Medal, awarded this year by the Council of the Society to yourself, "for the valuable services rendered by you to geological science, and particularly for your Geological Map of Ireland, the result of your own laborious and judicious researches."

It has been my fortune to go over no small proportion of the geology of Ireland, and to visit districts in the sister kingdom of the structure of which our information was wholly derived from your labours. I can therefore bear personal testimony to the value and extent of your researches, and express, from my own knowledge of the facts, the admiration that I feel for one of the most remarkable geological maps ever produced by a single geologist. The more your country is explored the more will men of science be astonished by the minuteness of detail contained in that famous work. You have investigated a country that in great part was, previously to your labours, a geologically unknown land. The formations with which you had to deal presented in many places peculiarities that demanded original thought, and precluded the usually safe guide of analogical comparison. You bravely grappled with your difficulty and showed your powers of generalization and systematization in an arrangement and nomenclature of the Irish rocks, which in many points were highly original, and have been of great use to labourers in the sister kingdom. These will be of more use still, and every year's research convinces those who are now officially engaged in the exploration of Ireland—Sir Henry De la Beche and Mr. Jukes, who are the best witnesses, can bear out what I say—that your great work is a surprising monument of observation and skill. The terms "calp" and "yellow sandstone" are important geological divisions of your founding, and you first showed that the Old Red of Ireland was not Silurian. Let me thank you too for the constant attention that you have paid throughout your labours to the observation and collection

of organic remains, and the accuracy with which, in every case, their geological and topographical positions were noted by you. The collection you made of Irish fossils is the finest of proofs of your success, and the works descriptive of them by Professor M'Coy, issued under your auspices, and distributed by your liberality, are most valuable contributions to palæontological science.

In thus addressing you I speak reverentially to one of the earliest members of this Society, and to a geologist who appeared in print before I was born. From the commencement of your labours the economic bearings of the science were kept constantly in view by you, and your earliest memoirs are upon the coal-fields of Ireland,—that on the Leinster coal-field having been published so long ago as 1814. You proposed, in a letter to the Dublin Society, to construct a geological map of Ireland as long ago as 1821. I need not say, on this occasion, how you have since carried out that wise proposition. Twice President of the Geological Society of Ireland, your spirit and presence has done much to promote the study of our science in your country. May your good influence long continue.

Dr. Griffith replied in these words :—

Mr. President,—I receive, with much gratification, the Wollaston Medal for the present year, which has been conferred upon me by the vote of the Council of the Geological Society.

It is an honour to which I never aspired, but which, I confess, I receive with pride and satisfaction, as a proof that my labours in the geological field of Ireland are appreciated by those who are best able to judge of their accuracy and importance.

The construction by me of a Geological Map of Ireland is now an old story, as upwards of forty years have elapsed since I commenced it at the pressing instance of Mr. Greenough, one of my oldest and most valued friends. It was in the summer of the year 1812 that the first outline of the Geological Map was attempted, when, to my own then limited observations, were added the hasty notes of my friend Mr. Greenough. Since that period I have never lost sight of the work, though public avocations have occasionally so much interfered with its progress, that only trifling additions to the general data were made during several years.

The meeting of the British Association for the Advancement of Science, at Dublin, in the year 1835, gave a fresh impulse to my labours, which from that time to the present have never flagged; and I have now the pleasure of presenting the result of my labours in as complete a form as time and opportunity have enabled me to produce.

The Topographical Map, on which the geological boundaries have been engraved, was constructed at the Ordnance Survey Office, Dublin, under the superintendence of my friend Major Larcom, Royal Engineer, in the year 1838, and is the most accurate Map of Ireland that has hitherto been published. It is laid down to a scale of four miles to an inch, and, although on that scale it appears in many parts to be crowded with geological details, yet, even in those places,

if we look to the scale of nature, it is meagre indeed; in fact my labours must be considered only in the light of an outline and precursor, which may facilitate the labours of Sir Henry De la Beche and his unrivalled corps of zealous and enlightened colleagues, who, in the true spirit of English scientific honesty, will at all times award the full meed of credit due to an antecedent labourer.

In a work which has been forty years in hand, during which time Geology has made such rapid strides, as might be expected, the first outlines and divisions into systems have been frequently revised and changed, and I may say, I have found it necessary to revisit every district, nay nearly every parish, in Ireland at least three times.

I may allude to the subdivisions of the Carboniferous system, from the Old Red Sandstone, or Devonian, to the Coal-formation inclusive: this system, originally given as one undivided suite, is at present subdivided into seven series, five belonging to the Carboniferous limestone and two to the Coal. I now look back with wonder at the labour, and perhaps danger in a scientific view, of attempting subdivisions, some of which at the time were new to geologists.

Next I may point to the subdivision, or at least attempted subdivision of the Slate-series, formerly called Transition or Greywacké slates, into Cambrian and Silurian, rendered necessary by the wondrous labours of my friends, Professor Sedgwick and Sir Roderick Murchison.

But I fear and feel I am trespassing on valuable time, and shall conclude by thanking you, Mr. President, for the very flattering manner in which you have had the kindness to express yourself in presenting to me the Wollaston Medal.

On presenting to S. P. Woodward, Esq., the Proceeds of the Wollaston Fund, the President addressed him as follows:—

Mr. Woodward,—It is with no small pleasure that I announce to you that the Council of the Geological Society have awarded to you the proceeds of the Wollaston Fund for the present year. They do so "in acknowledgement of the value of your recent Palæontological labours, and to assist you in the publication of your researches into the structure and affinities of Brachiopoda and Rudista." You and I have worked together officially within these walls, and I know well how thoroughly you have mastered the details and the generalities of those branches of Palæontology to which you have directed your attention. Every naturalist who has made himself acquainted with the admirable manner in which you have worked out the difficult palæontological problem of the structure and affinities of the Rudista, and how many anomalies have been definitely cleared away by your labours, will be glad to hear of this award. We trust that the results of your researches will before long be made accessible through publication, and that you will continue to pursue a course which is sure to gain you honourable fame and secure for you a high position in the world of science, worthy of the son of one who did much good service to British Geology in his time.

Mr. Woodward replied—

Sir, I beg to thank you, and the Council, for the valuable compliment you have conferred upon me. It is well known to many gentlemen now present, that I commenced life in the service of this Society, as assistant to Mr. Lonsdale, whose name is still so highly esteemed within these walls; and I enjoyed the good fortune of remaining here during the whole time of your former official connection with the Society. In the position which I have held, it has been my duty rather to assist others than to conduct investigations of my own; and I have been amply rewarded by the kind and liberal manner in which the slightest services have always been acknowledged by Fellows of this Society.

ANNIVERSARY ADDRESS OF THE PRESIDENT.

Gentlemen,—Whilst we rejoice in our continued prosperity and look forward confidently to the future progress of Geology, we must not forget, that the year just gone by has been one of mourning for science and of heavy losses by death, abroad and at home, for our Society. First in the list of the departed, who were among our eminent members, is the name of

Leopold Von Buch. The death of this illustrious philosopher and pre-eminent geologist took place, after a few days' illness, at Berlin, in March 1853. He had attained the age of 79 years, and to the last preserved his unrivalled energy and scientific enthusiasm.

Baron Von Buch was a member of an ancient and noble Prussian family, and was a Royal Chamberlain of Prussia; knighthoods and distinctions of all kinds had been showered upon him unsought, for his merits. He was one of the eight Foreign members of the Institute of France, and a foreign or honorary fellow of almost every great scientific academy out of his own country. At home he was one of the most active members of the Berlin Academy of Sciences. Fortunate in the possession of a sufficient, if not ample income, untied by the trammels of office or routine duties, he was enabled to devote the whole of his long life to the search after scientific truth. Nobly did he fulfil his mission. Unselfish, free from envy, anxious and able to aid, he sought not only to advance science by his own exertions, but to assist by advice at all times, by purse where necessary, every younger man who worked earnestly in the same course. There is an old Jewish proverb which says, "He who seeks a name loses fame;" Leopold Von Buch scorned fame and gained it.

He was a pupil of Werner; one of the youths destined afterwards to be illustrious, who studied under the instruction of the renowned professor of Freiberg. However serious the demerits of many of the views promulgated by that distinguished teacher, his eloquence and inspiration effected mighty services for geology, through the love for the science with which it imbued his disciples. Errors vanish in the course of time—they are like unpreservable species in geolo-

gical formations,—but merits last for ever, for through them science cannot fail to advance. Von Buch was one of the first to repudiate the mistaken views of his master, but he avowedly did so by the very spirit and method of research which he had cherished and learned at the school of Freiberg.

Von Buch was only eighteen years of age when he commenced his long series of contributions to the literature of science. His first paper was "A Mineralogical Description of the Carlsbad Region," printed anonymously in 1792. Four years afterwards he produced his "Contribution to a Mineralogical Description of Landeck," and soon after a similar treatise for Silesia, accompanied by a geological map. His merits as an accurate observer and clear describer were manifested in these early productions.

In the now venerable and ever-illustrious Humboldt he found a friend and fellow-student, with a kindred mind and genius, and these two great men worked together early in life. At the close of the last century they visited the Alps and Italy in company, and there it was that Von Buch commenced those researches into the geological phænomena of volcanoes that alone would have immortalized his name. He founded a great part of what may be termed the Science of Volcanoes, and gradually divesting himself, by the legitimate process of extended observation, of Wernerian theories, worked out this most interesting section of geology in the countries most likely to enable him to solve the many problems it presents. Italy, Central France, the trap districts of Germany and Scotland, and eventually, in 1815, the Canary Islands were submitted by him to close personal inspection; with what results I need not, in this place, recall. Suffice to say, that his great work, 'The Physical Description of the Canary Islands,' will long remain an enduring monument of his labours and his generalizations. The theory of Craters of Elevation was one of the most influential of the doctrines broached by him after his careful and prolonged study of igneous phænomena.

But during the course of these peculiar studies his mind was not confined to them, and other subjects of equal importance engaged a portion of his attention with as valuable results. In 1806, whilst Europe was torn by revolutions among men, Von Buch retired to the wilds of Scandinavia, there to study the greater revolutions of Nature. During a two-years' travel in Norway, Sweden, and Lapland, his inquisitive spirit did not fail to evolve new subjects for its speculations. What he saw there forced him to abandon the belief in the necessarily primitive date of granites, and his observation of the gradual rise of the Scandinavian area and its attendant phænomena, previously only imperfectly noticed and quite misunderstood, has been a fruitful source of fresh chapters in geology. How many of the best disquisitions of our time could trace their roots to these observations of Von Buch!

The first Geological Map of Germany appeared in 1824. Von Buch's name is not appended to it, but it is known that he was the compiler and author. The impulse to local geology given by a first map, and the difficulties with which the constructor has neces-

sarily to contend, cannot be too highly appreciated. The first step, in this as in many other things, is the chief difficulty, and one apt to be underrated by those who come afterwards.

After his return from the Canaries, Switzerland,—always the favourite region with Von Buch,—again became the scene of his travels. The mode and epochs of the upheaval of mountain chains were, among other subjects, the themes of his inquiries and essays. The famous doctrines of Elie de Beaumont bear witness to the influence and suggestiveness of Von Buch's observations. His theories concerning Dolomite, though not so productive of rich results, excited general attention and caused much wholesome controversy.

Twenty-five years ago, when already past the fiftieth year of his age, Von Buch seemed to enter upon an entirely fresh career, and to take up a line of inquiry in a totally different direction from that which he had previously followed, for he commenced those brilliant palæontological researches that have secured for him a permanent fame among the cultivators of the natural history side of geology, and even among pure naturalists. I say "seemed" to enter upon this course, for the thought and study had long been working in his mind, as is evident from the essay 'On the Progress of Forms in Nature,' printed by him as early as 1806. The ideas, then conceived imperfectly, had been silently and steadily growing within him, nourished by continual observations, and in 1828 they took a definite form, when he published his observations on Ammonites, followed at intervals by his monographs upon the Goniatites, Brachiopoda, and Cystidea. There are two distinct aspects of Palæontology, a geological side and a physiological side. Cuvier was the true architect of the latter, but Von Buch erected the former. It was he who first developed the idea of the chronomorphosis of genera, the great leading principle of natural history applied to geology. He arrived at it fairly and inductively, and demonstrated it monographically and practically. He gave a grand impulse to the study of stratified rocks, an impulse only now beginning to be felt in its full force. With his usual sagacity he saw clearly its value and bearings, as is plainly indicated by his essays on comparative or geographical geology, and the latest of his numerous memoirs, those on the Cretaceous and Jurassic formations. If I am not greatly mistaken, the future progress of comparative geology will depend mainly on the following up of the palæontological doctrines that were originated by Von Buch. Viewed, too, entirely apart from their geological merits, and considered under a purely natural-history aspect, the monographs on fossils by Von Buch are most remarkable productions, both as descriptive and as philosophic essays. Not long before he died he directed his attention to fossil botany, and endeavoured to evolve guiding principles from the study of the nervation of leaves. He did not rashly enter upon this fresh subject, for botanical inquiries had long before interested him in their details, as his Scandinavian and Canarian researches testify.

Philosophers may be divided into two great natural orders, those who sow and those who reap — the originators and the demon-

strators. Von Buch was a sower. He went about the world casting the seeds of new researches and fresh ideas, wherever his prophetic spirit perceived a soil adapted for their germination. The world of science has gathered a rich harvest through his foresight. He is the only geologist who has attained an equal fame in the physical, the descriptive, and the natural history departments of his science. In all three he has been an originator and a discoverer. In every subdivision of all three he has been a suggester — a high merit in itself.

Von Buch never married. Personally he had his peculiarities and eccentricities, odd ways of his own that amused the stranger and endeared him to his many friends. Probably no geologist had ever so general an acquaintance. He went everywhere to take the measure of the workers in his favourite science, and knew them, bodily and mentally, almost all. I shall ever esteem it a good fortune to have seen him, to have received a lesson from him, and to have deserved his published commendation. Though gone from among us, his ubiquitous spirit is with us, and in the Report which I shall have to give of geological progress during the past year, I could point out the influence of his ideas at almost every step.

A short half year has passed away since among the most active and vigorous of our younger members, Hugh Edwin Strickland took a prominent part in our meetings and discussions. Healthy, earnest, and indefatigable, his life promised to be one of long services to natural science. In the best period of manhood, when experience and energy meet and work together, when well-sustained exertions in the cause of truth, and the proofs of an equal capacity for scientific learning and original research have raised our expectations and cherished our hopes, Mr. Strickland was taken from amongst us awfully and immediately, falling literally a martyr to geological science. He had been engaging, with his customary zeal, in the discussions of the Meeting of the British Association at Hull, when at the termination of the sittings he proceeded to the neighbourhood of East Retford, to examine the cuttings on the line of railway at the mouth of the Clanborough Tunnel. Intent upon his observations, note-book in hand, unhappily unaware of the danger of his position, he stepped from one line to another to avoid an approaching coal-train, just at the moment that the Great Northern passenger-train was issuing from the tunnel. Instantaneous death terminated his earthly career.

Mr. Strickland was in the forty-second year of his age. He was a native of Righton in Yorkshire, and inherited scientific tastes from his father, Mr. H. E. Strickland of Apperley, and his maternal grandfather, the eminent Dr. Edmund Cartwright. Part of his education was conducted by the late Dr. Arnold, who took a warm interest in the talents of his distinguished pupil. His training was completed at Oxford, where he studied at Oriel College, and where doubtless, under the lectures of Dr. Buckland, he acquired and ripened

those geological tastes which eventually led to his appointment as successor of his illustrious master in the Geological Chair of the University.

His acquirements in natural science were singularly diversified, embracing more or less all departments of natural history, and there are few sections of the science upon which at one time or other he did not publish observations or memoirs. His knowledge of the literature of natural history was remarkably extensive, exceeding probably that of any living naturalist; and the 'Bibliographia Zoologiæ et Geologiæ,' based on the manuscripts of Professor Agassiz, and published by the Ray Society, owes much of its value to his editorial care and unrivalled acquaintance with authors and their works. As a zoologist, the greater share of his attention was devoted to ornithology, in which department he enjoyed a world-wide fame. His work, written jointly with Dr. Melville, on extinct birds, especially the Dodo, was an application of his ornithological knowledge to geology.

Mr. Strickland's geological researches were confined to no single locality or group of formations, and his name will be ever recorded in histories of geology. In the British Islands his favourite subjects were the New Red Sandstones, Lias, and Pleistocene beds. In conjunction with Sir Roderick Murchison, he described the geology of the neighbourhood of Cheltenham, and communicated to our Transactions the well-known important memoir "On the Upper Formations of the New Red Sandstone system in Gloucestershire, Worcestershire, and Warwickshire." Many papers on the geology of various points in these counties are contained in our Journals and Proceedings. He contributed also to a knowledge of the geology of portions of Scotland and of the Isle of Man. In company with Mr. Hamilton, he travelled in the Mediterranean and Levant, and explored the geology of parts of Asia Minor, the Thracian Bosphorus, and the Island of Zante. Their joint memoirs on those countries are printed in our Transactions, and contributed materially to extend our knowledge of the structure of Eastern Europe and Western Asia. The demonstration of the existence of Palæozoic strata on the shores of the Bosphorus was one of the many fruits of this expedition.

Earnestness, energy, and simplicity were the distinguishing features of Mr. Strickland's character. He was thoroughly a man of science, and as thorough a gentleman. Fearless in his maintenance of his convictions, whether by speech or pen, his freedom from animosity and evident straightforwardness invariably converted his opponents into friends. Mr. Strickland was most happily married, but has left no family. His father-in-law, the eminent naturalist Sir William Jardine of Applegarth, has undertaken to complete the editing of the remaining volumes of the 'Bibliographia.'

One of the warmest and wisest friends of the Society, and during many years an active member of it and constant attendant at its meetings, was Charles Stokes, whose name will be long borne in mind with affection and gratitude by many geologists and naturalists. Although constantly and assiduously engaged in business, Mr. Stokes

contrived, whilst passing his days in the City and on the Stock Exchange, of which he was a most respected member, to acquire a vast amount of minute and accurate scientific information, and to pursue original, though, alas, too seldom published researches; and there was scarcely any department of the natural history sciences with which his acquaintance was not considerable. Careless of fame and brimful of benevolence, he laboured incessantly, whenever a moment of leisure permitted, to advance science by every means that lay within his power. He collected rare and interesting specimens at any cost, not for their own sakes, but to place at the disposal of any competent person who had the requisite knowledge and determination to investigate the subjects they could serve to elucidate. Before microscopic science was in fashion, he was at work encouraging the makers of microscopes, suggesting improvements, purchasing beautiful instruments, and testing their application. When lithography was in its infancy in England, he foresaw what could be done with the rising art; and, sparing no expense, found a zealous and talented ally in the late Mr. Hullmandel for experimenting on his suggestions. His knowledge of some branches of zoology and palæontology was minute and curious, as well as of parts of botany. Trilobites and Zoophytes were among his favourite subjects; upon the former he communicated valuable materials and information to the great work of Alexander Brongniart on the Fossil Crustacea; about the latter he possessed a store of novel and original information, which I fear is in great part lost with him. The subject of the fossilization of wood was one which he pursued even to the last; and only two months before his death I received a letter from him, accompanying some specimens illustrative of his views, and inquiring about others. In the 5th volume of the 2nd series of our Transactions is published a valuable paper by him on this subject, containing an explanation of the phænomena exhibited by partially silicified wood, and of the progressive steps in the process of petrifaction. In the same volume is a memoir upon "Some Species of Orthoceratа," with an account of the siphon of Actinoceras and the foundation of the genus *Ormoceras*. The many curious researches concerning the Orthoceratites that have interested palæontologists of late years had their origin in his discoveries. Some time before he had made mineralogical communications to the Society. His name is constantly cited in numerous foreign treatises. But the scantiness of his writings can give no true notion of his learning and his influence on the progress of science during his time. Not an expedition started for foreign discovery, but he was in at the commencement to advise and direct the natural history arrangements. I am one of many who owe much to the sound sense and surprising knowledge of Charles Stokes. He was the Ellis of our times. I have spoken only of his scientific learning; he was as remarkable for literary, antiquarian, musical, and artistic knowledge. He died in London, deeply regretted, in the last week of December 1853, at the age of 70. His pleasant and wise presence will be missed for many a year.

Mr. Alexander Robertson, whose name as a geologist was best known to us with the affix "of Elgin," was born at Aberdeen in the year 1816, and after an education, conducted partly in England and partly in Scotland, became a pupil of Professor Syme, and studied medicine at Edinburgh. Disliking his intended profession, he soon abandoned it, and after studying science in Germany, returned to Scotland and settled in Morayshire, to pursue farming, I regret to say without success, since his death left a widow and four children unprovided for. Mr. Robertson's name is familiar to you as that of the discoverer of Freshwater beds intercalated in the Oolites of Brora. He communicated his observations on this subject to our Society, and subsequently (May 1846) a longer memoir "On the Wealden Beds of Brora, Sutherlandshire, with Remarks on the Relations of the Wealden Strata and Stonesfield Slate to the rest of the Jurassic System, and on the Marine Contemporary of the Wealden Strata above the Portland Stone;" an essay remarkable for its thoughtful and suggestive character. In this paper he pleaded forcibly for Sir Roderick Murchison's view of the Oolitic relations of the Wealden; some recent discoveries have in great part supported the argument maintained by Mr. Robertson.

Mr. Henry William Taylor was well known to the Members of our Society for his fine collection of Chalk fossils, which he spared neither time nor expense to bring together.

I shall not venture, Gentlemen, in the following Address to discuss fully and in all its details any one subject, in the manner so admirably and usefully done by my immediate predecessor, but follow the plan pursued by not a few of the distinguished men who have filled this Chair, of presenting to you in brief a summary of the leading features of geological progress during the year just passed, and a commentary on the aspects and aspirations of our science as manifested and indicated by the more salient labours of geologists during 1853. To do this thoroughly would require more leisure and a greater command of foreign languages than, unfortunately, I possess; but without professing to furnish a complete report, I hope to be useful by indicating the merits of that which has principally been done during the period I have had the distinguished honour of filling your presidency. If I claim the privilege of occasional criticism and difference of opinion, the responsibility of objections must fall entirely on myself, and if, through inadvertence, I commit injustice, by passing unnoticed any essay of merit and consequence, I trust on a future occasion to rectify the mistake and to render the acknowledgement that is due.

That the greater part of my report will take cognizance of Geology under its palæontological aspects, is a circumstance not dependent on my own predilections or peculiar line of study; it so happens that the majority of important papers published during the past year have been more or less of this character, and some of the most valuable of recent contributions to our science concern principally the natural

history department of Geology. The economics of the science have, it is true, received more than their usual share of attention, gold and coal forming the themes of not a few volumes and essays. But geology, properly so called, of a scientific character, is thinly diffused through auriferous treatises, although a great deal is often written in them about geology,—in the sense however of *round about* it. Descriptive Geology is constantly progressing, although the number of memoirs in this department has not been great during 1853. Knowing how much is in progress in this most important section of the science, we cannot regard the deficiency in the number of publications upon it as any indication of halting. The same remark may be made on the section of Geological Dynamics. Mineralogy, under its geological aspects is making decided progress in France and in America, but, to our shame be it said, continues to be neglected in England. There are numerous cultivators of it, it is true, for its own sake, learned and able mineralogists, who however, on this side of the Tweed at least, do not often put the results of their observations into print. In Scotland and in Ireland the pens of the mineralogists are much more active, and the investigation of mineral species ardently pursued, though not to the extent that we find these inquiries followed in Germany and the United States.

In the course of study of the many lately-published memoirs from which the materials of my Address are derived, the question of the meaning of the difference and contrast that are evident when we compare the faunas and floras of the more ancient or palæozoic with those of later epochs, has, in consequence of fresh accumulation of relevant facts, forced itself vividly upon my attention. It is a subject that, in common with most geologists, I have often earnestly thought over, and more than once published opinions upon. It has been the originator of not a few theories and speculations, not one of which can be said to have borne the test of searching inquiry into facts. Yet I think I am not wrong in saying, that a belief is as strongly impressed as ever on the minds of geologists who take interest in the philosophy of their science, that some law lies at the foundation of this difference. If I venture to add one speculation more, although its predecessors have either subsided into azoic oblivion, or linger, retaining but a weak hold upon our minds, I do so in the hope that there is a vitality in my offspring, which may enable it, when it becomes developed, though as yet only a suggestion, to endure; and I ask your indulgence for introducing it on this occasion, on the plea that it owes its birth to reflections arising out of this discourse.

The publication of the first volume of M. Barrande's great work on the Silurian System of Bohemia is a leading event of the geological year just completed, and from its importance commands our first attention. The researches, the results of which are embodied in this elaborate and beautiful treatise, were commenced twenty years ago, but have been more especially prosecuted during the last thirteen years. From time to time we have had more or less detailed notices of the fruits of M. Barrande's assiduous labours, but could

scarcely judge of their minuteness and importance until he commenced to send them forth in full. He now takes his place definitely in the foremost rank of geologists and palæontologists. He combines in a remarkable degree both qualifications,—no small advantage when the wide general views and the classification of great formations, such as are dealt with by this eminent man, have to be fully considered and put forth with ample arguments. Division of labour is good for the accumulation of sound and abundant materials, but experience has shown in both geology and the other sciences, that the greatest advances are to be made by combinations of kinds of knowledge in those who deal with the greater problems. M. Barrande has done well, it seems to me, by pursuing assiduously the double course he has chosen. The main body of the purely geological portion of his work he proposes to publish when the palæontological details, which constitute most of the evidence upon which his views are founded, have been laid before the geological world in all their completeness. The task he has before him in this respect is a laborious one; no less than the detailed description and critical investigation of some 1200 species of fossils,—for such is the number that has rewarded his search in Bohemia. The natural history and principal part of his first volume, a bulky work in itself, is devoted to the order of Trilobites. It is prefaced, however, by a general outline of the geology of Bohemia, which first deserves our notice, both on account of the interest it must present to British geologists dealing with palæozoic strata, and also because of certain original and peculiar views put forth in it.

The Silurian formation of the centre of Bohemia constitutes a well defined basin of an elongated oval shape, the great axis of which is directed nearly N.E. and S.W., and has a length of about 20 German geographical miles with a maximum breadth of 10. It is from 55 to 60 miles in circumference. Towards the N.E. and N. a small portion is bounded by the Trias, the Quader-sandstone, the Planer-kalk, or by the Carboniferous formation. Elsewhere, for two-thirds of its margin, granite or primordial crystalline rocks, such as gneiss and mica-slate, constitute its base and its boundary. A few small carboniferous basins are sprinkled over the Silurian surface, as well as a few isolated outliers of cretaceous beds. The dip in the two halves of the basin, the one to the N.E. and the other to the S.E. of the chief diameter, is towards the principal axis. The beds ordinarily lie at an angle of from 30° to 45°, often 70°, and are not unfrequently vertical.

M. Barrande distinguishes eight stages of strata to which he assigns a Silurian age; four of them he regards as Lower Silurian, and four as Upper Silurian. Of his Lower Silurian stages the two lowermost are azoic, the distinctions between them being founded on mineral characters, the first being composed of crystalline rocks, and the second of clay-slates and conglomerates, similar to the fossiliferous Silurian above them, but wholly void of organic remains. They are rich in lead mines. These azoic stages pass into each other, and the upper section passes gradually into the fossiliferous beds above.

The third stage of his Lower Silurian, and the first of his fossiliferous horizons, includes his "Schiste protozoique," and attains a thickness of 1200 feet. It contains no beds of limestone. The fauna of this section is very peculiar; it is composed almost totally of Trilobites, the other fossils being a Pteropod, some Cystideæ, and an Orthis. These constitute an assemblage upon which he lays great stress and designates *primordial*. All its species, without exception, are peculiar to itself, and of the Trilobites, all the genera are so, with the exception of *Agnostus*. The peculiar genera are either low and rudimentary types, or members of the Olenoid or Calymenoid families; not typical or highly developed forms. They are *Paradoxides, Conocephalus, Ellipsocephalus, Sao, Arionellus, Hydrocephalus,* and *Agnostus*. Of the first of these genera there are no fewer than twelve species, some of them exceedingly prolific. These primordial Trilobites have a peculiar facies of their own, dependent on the multiplication of their thoracic segments, and the diminution of their caudal shield or pygidium. M. Barrande compares this primordial fauna of Bohemia with certain fossiliferous assemblages similarly placed at the base of the fossiliferous Silurians in Wales, Norway, and Sweden, in which last country, indeed, the peculiarities of its fossils long ago attracted the attention of naturalists and the notice of Linnæus.

The isolation of this primordial zone, as distinguished from the mass of the Lower Silurian, is chiefly maintained by the grouping in it of the Olenoid family of Trilobites, almost to the exclusion of all others. It is not quite certain that more than one of the genera of Trilobites distinctive of this zone are found in any higher beds. The exception is *Agnostus*, the lowest and most rudimentary type of its tribe. Yet even this has its metropolis in the primordial zone, and sends but a few stragglers into the division immediately above. The same, or a very similar distribution, has been observed of late years by Angelin, who during 1852 commenced illustrating the fauna of the Swedish rocks.

In Wales the existence of this primordial fauna has been clearly made out. The rocks which contain it are those designated by Professor Sedgwick, who recognized their importance, as the "Lingula beds," a name adopted by the Geological Survey. Fossils were first I believe, found in them by Mr. Davis, who discovered the *Lingula*, from which they received this name. They have been thoroughly examined by my colleagues of the Geological Survey, and are stated in the *resumé* on the Lower Palæozoics of N. Wales, communicated by Professor Ramsay to the Society last April, to be about 7000 feet thick. Their importance has been fully recognized for some time by the surveyors, and the additional evidence accumulated last autumn by Mr. Salter goes to support the stress laid upon them by M. Barrande. In the prosecution of the search, a further result has been obtained in the way of a subdivision of the group, and a palæontological distinction of importance has been indicated. They prove capable of division into two well-marked sections, viz. a lower, of which *Agnostus* (probably the identical species described from the alum

slates of Sweden), an *Olenus*, and *Conocephalus* occur along with the characteristic *Lingulæ* of the deposit; and an upper, where the same genera are accompanied by a few Brachiopoda and Bryozoa, as in Bohemia. But whereas in the latter country no passage can be shown of this fauna into the Silurian stage above, in Wales a palæontological passage from the Lingula beds into the Bala or Llandeilo group appears to be indicated. This is marked by the association in the upper part of the igneous series of two large species of *Olenus* with *Agnostus* and *Lingulæ*, and with types unquestionably characteristic of the Llandeilo beds, such as *Asaphus*, *Calymene*, and *Ogygia*, and Graptolites of species undistinguishable from those of the Llandeilo flags.

The interesting memoir of Dale Owen leaves no doubt upon the equivalence to these beds of the Potsdam sandstone of North America, in which Trilobites of the Paradoxides type are mingled with the Lingulæ, so characteristic of this formation.

The demonstration of this important zone of life, the earliest as yet distinctly traced, is a great step in Palæozoic Geology, one firmly established during the past year.

The extinction of the primordial fauna in Bohemia is attributed by M. Barrande to the effects of the igneous eruptions manifested by the masses of porphyries interposed between his lowest fossiliferous and the succeeding stage. The destroying influence of trappean eruptions are more than once laid stress upon by him. Similar phænomena appear to have terminated the Lingula-flag epoch in the Welsh area; volcanic outbursts, as remarked by Professor Ramsay, "in consequence of which great ashy deposits were found interstratified with ordinary muddy sediment, and here and there associated with thick beds of felspathic lava." But these outbursts do not always appear to have had so decided an influence upon the faunas of ancient seas, for, in the instance of Wales, the great eruptions that occurred during the epoch of the deposition of the succeeding Bala beds did not materially affect the population of the oceanic area in which they broke out. For my part I am strongly inclined to think that the influence of volcanic outbursts upon life through the destructive agency of the products of eruptions has often been overrated. Igneous overflows, showers of ashes, and exhalations of deleterious vapours are necessarily destructive, but as necessarily local, and scarcely likely, arguing at least from all cases of which we have sufficient knowledge, to extinguish the fauna and flora of a whole natural-history province, much less of many provinces. But they are the certain indications of far more powerful though less conspicuous and less traceable enemies of life. They are often the indices of epochs of excessive disturbances of the earth's crust, and of elevations and depressions of the surface of the sea-bed. Changes of level and consequent changes of surrounding conditions, even to the extent of change of medium, are the great life-extinguishers. The degree of substitution in an ancient fauna should rather be accepted as an evidence of the extent of the movements that have taken place during an age of volcanic energy, than as a measure of

the intensity of the local outbursts, the products of which at first glance seem to us the most efficient engines of destruction. It is not the ferocity of battles, but the organic changes among nations that afford us the measure of value or importance of a great war.

The fourth and uppermost division of M. Barrande's Lower Silurians is his "Etage D;" strata chiefly composed of quartzites with schistose alternations. Cephalopoda represented by *Orthoceras*, Pteropoda by *Conularia* and *Pugiunculus*, Heteropoda by *Bellerophon*, Gasteropoda by *Pleurotomaria* and *Holopea*, Acephala by *Avicula* and *Nucula*, Brachiopoda by *Orbicula*, *Lingula*, *Spirifer*, *Leptæna*, and *Terebratula*; also Crinoids, Cystideans, Starfishes, and a few Corals and Graptolites make up, with *Trilobites*, the fauna of this group in Bohemia. Trilobites and Cystideans prevail above all other forms, and it is in this zone that the former (and the latter probably also) attain their maximum. This fact has a strong significance in its bearing on the hypothesis concerning the relation of Palæozoic life to the life of all after-periods which I shall hereafter bring out in this discourse. The assemblage of animals found in this stage of quartzites constitute M. Barrande's second fauna. He compares the stage with our Llandeilos and Caradocs, with the Lower Silurians of Ireland, Russia, France, Spain, Portugal, and Thuringia, the regions C and D of M. Angelin in Sweden and Norway, and the formations from the Potsdam sandstone to Hudson's River group inclusive, of the United States. The great geographical diffusion of its fauna is in accordance with its vertical extent. But though widely diffused as a well-marked fauna, exhibiting everywhere a characteristic and easily-recognized facies, the species are by no means universally diffused, and the resemblance between distant regions is maintained rather by representation than by identical forms: thus early in the world's history do we find the partitioning of the earth's surface into natural-history provinces. More and more evident does it become every day that the old notion of a universal primæval fauna is untenable, and that at all epochs, from the earliest preserved to us to the latest, there were natural-history provinces in geographical space. And indeed, if we consider for a moment upon what causes the existence of these provinces depends, how they are not the mere results of various climatal conditions only, but are regulated in their extent in the sea by orographical and hydrographical conditions, and on the land by the inequalities and arrangement of the surface, and thus call to mind that the vast and varying sedimentary accumulations, found at every epoch in great mineral dissimilarity, necessarily indicate the existence of those very inequalities and peculiarities on sea and on land that determine the existence and extent of geographical provinces and limit the diffusion of animal and vegetable species, it seems strange to us how the notion of the universal diffusion of a uniform specific fauna could ever have been accepted for a moment, even as an *à priori* hypothesis. It was imperfect recognition of the phænomena of *facies* in time, that beautiful idea that first seems to have dawned on the mind of Von Buch, which appears to have given rise to the error.

In one of the subdivisions of this "Etage D" of his Lower Silurians, M. Barrande describes the occurrence of isolated patches, as it were, of fossiliferous strata, the population of which consists not of Lower Silurian fossils, but of organic remains characteristic of the Upper Silurian. M. Barrande designates these assemblages by the appellation of "colonies." This colonial fauna, becoming extinguished after a short existence, does not reappear until after the extinction, by trappean eruptions, of the normal fauna of the epoch, and the cessation of the formations amid which the colony is an intruder. In these colonies, he states, there are as many as 63 species, of which 4 are exclusively peculiar, 2 (viz. *Trinucleus ornatus* and *Dalmanites socialis*) common to the colonies and the true fauna of the beds in which they are intercalated, and 57 common to the colonies and third fauna, or that of the lowermost section of his Upper Silurians.

This doctrine of colonies is original with M. Barrande and demands our serious consideration. It is one that materially affects the value of the evidence of organic remains as determining the age and sequence of formations. The proposition that it involves asserts the introduction of a group of species that experience has shown normally to belong to a later and distinct formation, not merely among and mixed with the fauna of an earlier stage, but amid and separate from that fauna. We can conceive, indeed we have ample proofs in many instances, of the fact of the appearance of many species earlier in one geographical region than another, and we can understand how under temporary favouring circumstances any one or a number of such species might be laterally diffused, so as for a time to become a component part of the fauna of a neighbouring region, at an epoch previous to that in which, after having for a time retired, they returned to play a more conspicuous and characteristic part in a later formation. But in any such instance they would be mingled with the ordinary inhabitants of the region they colonized. Yet we can scarcely conceive a colony, composed entirely of strangers and of species known in beds of a later epoch, only in the exclusive association presented by their being intercalated without admixture in the midst of an earlier fauna. On the other hand, in a disturbed Silurian country, where the strata lie at very high angles, and where there are probably numerous convolutions, contortions, and rollings of the beds, there is a probability of the occurrence of overturns and truncated crumplings, that until traced out would cause the appearance of strata containing newer fossils lying under and amid those containing older ones. Such deceptive appearances are not unfrequent in the Alps, and some well-known cases occur in our own country. With these instances vivid in our memory, I feel warranted in objecting to a theory which seems to me as dangerous as it is ingenious, and ask first for the minute local details of the course of the Silurian beds in Bohemia, before accepting a doctrine so repugnant to received belief.

M. Barrande, it is true, endeavours to show most ingeniously that the currents which determined the immigration of his colonies came from the N.E., and that the fauna of his Upper Silurians arrived by the same direction; whereas the fauna of the Lower Silurians of

Bohemia, if not created in place, arrived by currents having their origin in the S.W. If, however, as now suggested, contortions of the strata have deceived this able observer, an argument of this kind can have no weight.

Of the four stages of Upper Silurians in Bohemia, the three lower divisions are typically calcareous, and the culminant section schistose. The lowermost has a base consisting of traps alternating with black slates containing Graptolites, and including occasional concretionary limestones. It attains a thickness of not more than 900 feet, but has a fauna superlatively rich and prolific in fossil treasures. Between 500 and 600 species of organic remains have been collected in this formation. In it is found the maximum number of species of Trilobites, no fewer than 78; and several genera, including *Harpes*, *Bronteus*, and *Proetus*, appear for the first time in Bohemia. Cephalopoda abound; as many as 200 species, of which half are Orthoceratа, have rewarded the collector. *Ascoceras*, *Gomphoceras*, and *Phragmoceras* are the characteristic types. Gasteropods, Lamellibranchs, and Brachiopods are numerous, and there are not a few Zoophytes.

The second or middle stage of Upper Silurian limestones presents a decreasing fauna, but at the same time exhibits the maximum of Brachiopoda. Bryozoa and Tentaculites appear, and Cephalopoda rapidly diminish in numbers.

Between the third or upper stage of these limestones and the last there is a gradual passage, and in these fishes commence and Brachiopods have become rare. A considerable number of species in this division are enumerated as common to it and the two last.

In the uppermost stage of culminating schists the community of species is reduced to two Trilobites, and the entire fauna is poverty-stricken. Traces of vegetables indicate some considerable changes in the conditions of the sea-bed.

The four upper stages, constituting in their aggregate the Upper Silurians of Bohemia, contain a fauna (the third fauna of Barrande), which, as a whole, is regarded by its describer as of equal importance with the first or primordial fauna, and the second or chief Lower Silurian fauna. The strongest relations of identity of species between the Bohemian Silurians and those of other regions, are exhibited by the third or Upper Silurian fauna. A curious point concerns the second, viz. that it is represented in France not only by the same genera but also by identical species, whilst in England and Sweden it is represented by the same generic types and a great analogy of distinct specific forms. Of the different classes of animals it would appear that but few Crustacea are common to other countries, whilst the Cephalopoda, Brachiopoda, and Corals are widely diffused. The evidences of communication between the Silurian series of different regions are clearly indicated, and everywhere the distinction between his three great faunas is maintained by M. Barrande to be plainly exhibited. At the same time he pronounces definitely for the unity of the Silurian group as a well-characterized whole.

I would now call attention to the results of his inquiries into the

distribntion of Trilobites, and its bearings on the view of the arrangement and phænomena of the Silnrian formations, as stated above.

Ont of 45 genera of these typieally Silurian Crustaeeans, 35 are Bohemian, and of the 10 that are not so, 2 (viz. *Olenus* and *Peltura*) belong to the primordial fauna, not exclnsively however, for *Olenus*, in onr eonntry, aseends higher in the scries. Of the second fauna, 6 genera are not Bohemian. And ont of the entire list 7 genera have been reeognized only in Bohemia. Of the speeies of Trilobites, the number eharaeterizing eaeh of the stages goes on increasing from the primordial fanna to the lower portion of the Upper Silurians, but one speeies only is common to as many as four of his stages, and, a faet that is worthy of notice, varies in eaeh. The causes of destruction of speeies are not always clear. M. Barrande attribntes due inflnenee to physical ehanges as regulating their duration, bnt I must strongly protest against his belief in the doetrine of a limited vitality for each speeies ("nne quantité limitée de foree vital"), so that, independent of all other eireumstanees, eaeh raee will neeessarily beeome extinet after a eertain lapse of time. I have elsewhere exposed the groundless fallaey of this pernieious hypothesis,—a favonrite one with palæontologists, although it ean find but few physiologists to give it support. A curious remark is made by M. Barrande, that the speeies of the more aneient epoehs appear to have been more prolifie than those of later ages,—a remark doubtless suggested by loeal phænomena.

When eommenting on the general distribution of Trilobites in Palæozoie rocks, M. Barrande calls attention to the faet, that of the 44 Silurian genera, three-fonrths do not range upwards above the Upper Silurian stages; 11 reaeh the Devonian epoch, with notable dimiuution of speeifie riehness, and one only is found in Carboniferons rocks. The generie maximum of Trilobites is concentrated in the Lower, the speeifie maximum in the Upper Siluriaus. The direetion of the development of the Trilobites is as elearly baekwards, so to speak, iu time, as that of the Malaeostraca is forwards. The same remark may be made on the Brachiopoda as eontrasted with the Lamellibranchiata, and the Nantiloida as eontrasted with the Ammonitoida. On these oppositions I shall have more to say at the termination of this diseourse. Most worthy of remark is the fact confirmed by M. Barrande, that the geologieal position of a speeies in one region is not neeessarily that which it holds in another. This observation is independent of his colonial theory. Thus eertain Trilobites are common to the seeond and third faunas in England that are confined to one horizon in Bohemia, and others that are members of the Lower Silurian only in the British islands, are preseut in the Upper Silurian only in Bohemia.

An interesting point is the anamorphosis or ehange of eharaeters within genera in their eourse through time. M. Barrande's remarks on this matter are highly original and deserving of study. As instanees among Trilobites, I may eite the ehanges in the course of the grand suture; the progressive development of the eyes; the rednetion of the thorax; the inerease of the eaudal shield; the ehange

in the ornaments in the test, striation mainly preceding granulation. Features, however insignificant, of this kind chiefly give a distinguishing facies to the fauna of an epoch. Well was it said by Von Buch, that "the smallest difference acquires value by constancy."

I shall not attempt an analysis of the elaborate general zoological division of M. Barrande's work, or of the complete treatise on Trilobitic species that follows it. Suffice to say, that no student of Crustacea can be absolved from a close perusal of this most admirable monograph, and that every Silurian geologist should endeavour to understand and master its luxuriant details. In justice to the author, there is one section of this part of his work that cannot be passed without a remark, and that is, his chapter concerning the metamorphoses and modes of existence of Trilobites.

For years, ever since 1828, palæontologists have dreamt of Trilobitic metamorphoses, and some have pronounced definitely for, some as definitely against, the probability of the Trilobite undergoing changes in the course of its existence as an individual. The full discovery and statement of the fact was reserved for Barrande in 1849. In the same year Mr. Salter showed that the young individuals of *Ogygia Portlockii* presented 4–7 segments, and finally 8. Milne-Edwards and Burmeister, naturalists thoroughly versed in the history of living Crustacea, had previously speculated freely from analogy on the probability of their transformations. M. Barrande in the work before us demonstrates a metamorphosis in no fewer than 16 genera and 28 species. The degree of change is variable; its inteusity comparable with the phænomenon in existing Crustacea. The successive and progressive elaboration of all the elements in the pygidium before becoming free and passing into the thorax, holds good in all known metamorphosing Trilobites. The number of species in which a change has been proved diminishes as we ascend in time. Among other points M. Barrande has made out the probable eggs of these animals. As to their mode of life he opposes the conclusion of Burmeister and others, that Trilobites lived in shallow water along the coast; and distinctly pronounces against the supposition of their parasitic nature.

The Geology of the British Isles.

The well and often explored mine of British geology has not yet been worked out, and there are still rich lodes to discover, as well as old workings, that yield profitable returns when re-examined. Our Journal has had its full and usual share of papers on British strata during the past year, and, judging from what I know of memoirs in hand, the coming sessions are likely to be quite as well provided for.

Our Palæozoic rocks have received their usual share of attention. Old though they be, they are as attractive as ever, and their warmest admirers during preceding years remain constant to their antiquated yet ever fresh charms. The often discussed question of their classification has been made the subject of a communication by Professor Sedgwick to the Geological Section of the British Associa-

tion at Hull, explanatory of his views concerning the nomenclature of the Primary formations. The division of the Palæozoic strata into an Upper, Middle, and Lower series is a natural classification, although some may prefer a twofold instead of a threefold partition. The question concerning the appellations to be given to the subdivisions of these three sections, is one which will in the end be determined by custom and the authority of general use. Convenience is eventually the settler of all differences about nomenclature, and even in Zoology and Botany, sciences in which many definite rules are observed with laudable strictness, convenience every now and then overrides all our arbitrary regulations. Professor Sedgwick had previously, during the course of our last session, communicated, in association with Professor M'Coy, certain views of consequence concerning a proposed subdivision of the Caradoc sandstones, which demand a special notice on account of their importance, and because there have been more than one paper on this subject lately read before the Society. The result of these inquiries on the part of several observers is to place the relations of the Caradoc sandstone in a clearer light, both as to strata above and those below it.

The Caradoc was originally considered by Sir Roderick Murchison as the sandy and upper portion of the Lower Silurian strata. The rocks east of Caer Caradoc presented the best types, and those shown in ascending sections through what are generally called "the Pentamerus beds," to the Upper Silurian, and the arenaceous masses which occupied this position in the Malvern and May Hill districts, were considered by the founder of the Silurian system to be equivalents of at least a part of this series, while they graduated into the Wenlock shale.

But while our Caradoc sandstone, so constituted, contained in some parts numerous fossils that were Llandeilo species, in its upper portion it was supposed to contain these species mingled with the characteristic Pentameri. In America the latter fossils were found associated only with species characteristic of the Upper Silurians, and the group of strata containing this assemblage appeared to be cut off distinctly from the underlying Llandeilo rocks.

The unravelling of this anomaly is in part due to Professor Sedgwick, and in part to the officers of the Geological Survey. In a communication contained in the fourth volume of our Journal, Mr. Ramsay and Mr. Aveline have shown that the Pentamerus beds around the Longmynd repose unconformably upon the Llandeilo flags, whilst they graduated upwards, as Sir Roderick Murchison had stated, into the Wenlock shale. But here only the upper part of the Caradoc was developed, and this portion contained but few of the Lower Silurian species. In a subsequent paper in our eighth volume, the physical connection of the Upper Caradoc with the base of the Wenlock shale was definitely and fully stated. In the meantime, and quite independently, Professors Sedgwick and M'Coy examined the Caradoc beds of May Hill and the Malverns, and became convinced that these beds, containing as they did only Upper Silurian species, must be regarded as the base of the Upper Silurians, and that the Caradoc

sandstone, as then understood, comprised two distinct formations; that cast of Caer Caradoc (Horderly, &c.) being equivalent to the Bala rocks, while the group of May Hill, and probably the Coniston grits of Westmoreland, should be associated with the Wenlock and Ludlow series.

It became necessary for the officers of the Survey to test these views by an appeal to the county originally described, viz. Shropshire. The result of their labours is reported by Mr. Salter and Mr. Aveline, who undertook the task, in the first part of our tenth volume. They have shown that Professor Sedgwick's view is substantially correct, and that the typical district contains not only the equivalents of the Bala and Llandeilo rocks, but also the upper portion of the Caradoc, lying unconformably on the lower, and everywhere characterized by the Pentameri, and full of Upper instead of Lower Silurian species. These latter strata are therefore the exact equivalents of the May Hill, &c. beds. But although these rocks are thus evidently brought into a nearer comparison with the 'Clinton group' of North America and with the Pentamerus beds of Russia, they are still regarded by the Government surveyors as forming a bed of passage from the Lower to the Upper Silurians, inasmuch as several species which characterize the Lower Silurians are common in them, and especially since their distinguishing fossils, the Pentameri and Atrypæ, are found in certain portions of the Llandeilo flags, but are not known to rise into the overlying Wenlock strata. They propose to retain the name of "Caradoc sandstone" for these beds.

This evidence of intermixture of fossil species has received unexpected confirmation from America. In the second part of his 'Palæontology of New York,' Professor Hall has announced the fact, that a few of the most characteristic of the fossils of the Trenton limestones are now found in the upper part of the Medina sandstone, a formation as intimately connected with the Clinton group, as in our own country the conglomerates that skirt the Longmynds are with the overlying Pentamerus limestones and shales, and the analogy of these beds in the two continents is therefore complete.

Of the vast thickness and striking geognostic phænomena of our Lower Silurians, a concise but clear and most interesting statement is presented in Prof. Ramsay's paper "On the Physical Structure and Succession of the Lower Palæozoics of North Wales and part of Shropshire"—the prodromus of a more extensive memoir, now in preparation. These rocks, in the region described, include the prodigious amount of 42,000 feet of apparently conformable strata, including the Cambrian, in the sense in which the term is used on the maps published by the Geological Survey,—the Lingula and Bala series,—and the Caradoc sandstone. The grand facts of Silurian Geology will soon be presented in a complete and consistent picture by Sir Roderick Murchison, whose forthcoming work is anxiously expected, and is sure to fulfil all our anticipations.

The attention bestowed upon the Older Palæozoics of England has not of late been extended to the Middle and Upper. Through the

kindness of my colleague, Professor Ramsay, however, I am enabled to notice an important, though as yet unpublished, contribution to the geology of the Permian districts of the Midland Counties, one with considerable economic bearings.

In all existing published maps the actual upper limit of the Permian rocks south of Derby and North Staffordshire is merely guessed at. These beds are often inserted where they do not exist, and omitted over large areas where they should be inserted.

They have now been clearly mapped and accurately defined around the Tamworth, the Coalbrook Dale, the Forest of Wyre, the Shrewsbury, and part of the North Wales coal-fields. A large area has in consequence been taken from the supposed Bunter sandstone and mapped by Mr. Ramsay and Mr. Howell as belonging to the Permian rocks in the country lying between Tamworth and Leamington, in part of which, at Exhall, a Permian Calamite, and casts of shells having Permian affinities, have been found; by these means, then, geologists have been able to support palæontologically what previously was maintained by Professor Ramsay on purely physical grounds. These facts are also important, since they prove that the Labyrinthodon described by Dr. Lloyd in the Reports of the British Association (Birmingham) 1849, is a Permian reptile, and not, as he supposed, from the Bunter sandstone.

Through the course of last year important additions have been made to our knowledge of the Bunter sandstone, by working it out in four subdivisions in the districts that lie between Chester, the Abberleys, Warwick and Nottingham. Over large parts of this area there is found to be great constancy in the lithological character of these divisions, and by their aid the surveyors have been enabled to determine numerous faults hitherto unknown, which frequently repeat the same strata for many miles.

The supposed thickness of the New Red Sandstone will consequently be much reduced in places, and this, taken in connection with accurate measurements of the extent and thickness of the Permian strata, may at no distant date lead to important economic results, in the determination of the depth at which the coal-measures lie under large tracts of the New Red Sandstone area, where there can be little doubt that it will by and by be successfully sought for.

A great step has been made towards an explanation of some of the organic phænomena of the Oolites by Professor Morris, whose memoir "On some Sections in the Oolitic District of Lincolnshire," communicated to the Society in June last year, throws new and valuable light on the relations of the southern to the northern oolites in England, and rectifies several misconceptions about the comparative order of the strata in different districts. As this paper, one of the most important in its general bearings laid before us during the past year, is printed entire in our Journal, I shall make no abstract of its details, but merely offer a few remarks on its general bearings.

The marine faunas of the Oolitic epoch indicate at least three great and widely-spread assemblages of types, each exhibiting a general and easily recognizable facies. These aspects may be termed

respectively the Liassic, the Bathonian, and the Oxfordian ; the two latter terms being used for want of better, and being adopted in a wide and general sense, and not in the restricted meaning in which they are used by M. Alcide d'Orbigny. The horizon of change of facies at the boundary between each is a horizon, to a considerable extent, of change of species. I believe that every year's research will make it more and more evident that the perishing of species is simply the result of the influence of physical changes in specific areas, and depends upon no law of inherent limitation of power to exist in time. If so, we should expect to find indications of the cause of the greater changes in the oolitic and marine fauna in the shape of strata bearing evidence of a wide-spread change of physical conditions within the great oolitic area. An extensive change of species within a marine area in all likelihood is dependent on an extensive conversion of that area into a terrestrial surface.

Now it is becoming more and more clear that such a change of condition occurred over a very wide area in the interval between the main mass of the middle and upper jurassic types. The researches of Mr. Morris do much towards completing the demonstration of the nature and extent of these changes in the area now occupied by the British Islands, and it will be seen hereafter, how, even as far away as Italy, we have clear proofs of a similar change of conditions about the same epoch. Much may be done towards clearing up the details of this matter by more extensive and careful investigations of the Scottish oolites, guided by the light that is opening gradually upon us. Indeed I know of no field more likely to yield fresh laurels to the British geological observer than the thorough exploration of Scottish secondary geology.

In a paper by Professor Buckman, published in the 'Annals of Natural History' for November last, and one of the many interesting contributions to British geology which we owe to that active assembly of provincial observers, the Cotteswold Naturalists' Club, the Cornbrash and associated strata of the neighbourhood of Cirencester are described in detail, and under an economical point of view not always attended to, viz. the agricultural value of the soils formed by the several oolitic rocks. Through the predominance of phosphoric acid and sulphate of lime in the Cornbrash, as compared with the 'stone brashes' of the Great and Inferior Oolite, the value of the soils in the former rock is considerably greater, as shown by the analyses of Professor Voelcker. Mr. Buckman presents some good facts concerning the distribution of fossils in these beds, and enumerates twenty-one species of lamellibranchiate bivalves common to the Inferior Oolite and Cornbrash in Gloucestershire, and rare or wanting in the Great Oolite of the district. The recurrence of the species in this instance, as indeed in every similar case, is dependent on the recurrence of similar conditions. In every such case we may, *à priori*, assume that the intermediate strata, formed under different conditions, somewhere within the area of the ancient marine region to which they belong, change their character, putting on the mineral aspect and containing the peculiar fossils of the superior and inferior

beds. Phænomena like those recorded by Professor Buckman should therefore serve as pilot facts, and guide us to fresh discoveries.

The upper Mesozoic rocks of Britain have been of late left undisturbed, with the exception of that very moveable and problematical deposit, the sands and gravels of Farringdon, which Mr. Sharpe would elevate to a considerably higher position in the cretaceous series than has hitherto been assigned them. The time is probably fast approaching when the conflicting views upon this disputed question will be tested by fresh data. For the present I abstain from taking up this critical subject.

Our Tertiaries, on the other hand, have been treated with much favour, and form the subject of several memoirs, at the head of which stands Mr. Prestwich's account of the Woolwich and Reading series. This paper completes the series of memoirs by that eminent geologist descriptive of the Lower and Middle Eocene strata of England. These remarkable essays embody the results of many years' careful observation, and are unexcelled for completeness, minuteness of detail, and excellence of generalization. They have a further merit, and a very great one, to wit, that whilst in themselves essentially local and topographical, the examination of the British strata which they profess to describe has been conducted *pari passu* with personal comparisons and examinations of corresponding formations on the continent. This method of treatment, broad and catholic in its spirit, has made the essays of Mr. Prestwich as useful to foreign as to British geologists, and secured for their author a European renown. The special subject of the last of these papers is the series of strata constituting what is usually known as the Plastic Clay formation, the mutual relations of whose several local beds had never been clearly determined, and the relative position of the beds of the Reculvers and Herne Bay to those of Woolwich and Reading were quite unsettled. This condition of things can be said to exist no longer, and we have now, instead of confusion and uncertainty, a clear statement and correlation of the local phænomena at numerous points, with a thorough revision of the lists of organic remains, and most interesting generalizations respecting the geographical and dynamical changes that affected the area during the epoch under review. Since the memoir is printed at length in the first number of our Journal for 1854, I need attempt no detailed analysis here, or enter upon the many important questions and suggestions that are in it discussed.

It has been my own lot to investigate the fluvio-marine series that terminate the Eocenes in the Hampshire basin, and to lay before you a preliminary statement of the results at which I have arrived. The demonstration that a considerable and hitherto unplaced portion of these beds in the Isle of Wight represents the Limburg series of Belgium and the Upper Eocene or Lower Miocene of France, as well as other continental formations, of which we were supposed to have no equivalent in England, will, I trust, prove acceptable to all who take interest in Tertiary geology. Since I communicated my paper to the Society, I have revisited and carefully re-examined the fluvio-marines of Hempstead and those west of Yarmouth: also the sections

at Cowes and Osborne. At the latter locality, and there alone, the peculiar series to which I gave provisionally the inconvenient name of St. Helens, form a part of the surface of the island, so as to admit of being delineated on the map, for which reason I would, in accordance with the remainder of my nomenclature, designate them by the name of the district, and style them, in preference, the Osborne Series. Here also, in consequence of a considerable fault that runs in the course of the Medina, the Headon beds proper are brought up on the shore at East Cowes. A visit to the French tertiaries during last autumn has gone far to confirm the scheme of continental equivalents that I submitted to the Society, and the view which I maintained of the essentially Eocene affinities of our Hempstead and Bembridge series. I am inclined still to maintain that our succession of Middle and Upper Eocenes is more complete and continuous than that met with in either France or Belgium, the equivalents of our Bembridge marls and Lower Hempsteads being probably deficient in the former country, whilst those of our Headon series are absent in the latter. It is through the over-estimated value assigned to these breaks that the discordance in the opinions of geologists respecting the degree of relation between the Middle and Upper Eocenes in a great manner would seem to depend.

We owe to the Marchioness of Hastings an excellent detailed account of the Hordwell fluvio-marine section, the scene of the diligent researches during several years of that distinguished and zealous lady-geologist, whose contributions to British eocene palæontology have been among the most valuable and interesting made of late years.

The newer tertiaries and superficial deposits have received of late a considerable share of attention, but not more than they deserve. As yet we are scarcely in a condition to generalize upon them with safety, but are evidently fast advancing towards that desired point. Minute and repeated local observations constitute the soundest data for our guidance. Mr. Trimmer, who for years has devoted a considerable portion of his attention to this important, though not generally attractive department of geology, has communicated several papers to the Society, among which that constituting the third part of his essay, "On the origin of the soils which cover the Chalk of Kent," is peculiarly interesting and instructive. Mr. Morris and the Rev. Mr. De la Condamine have also contributed valuable notes. The whole subject may be reported upon as in progress, and, for the present, I reserve my comments. In the mean time, I would strongly urge upon British geologists the propriety of a careful comparison of the phænomena and features of the drifts, gravels, and superficial deposits of our southern districts with those of the neighbouring provinces of the continent. I believe that an investigation of this kind, which must be done personally, since continental memoirs scarcely afford sufficient data for the work, would tend to rectify many of our prevailing notions respecting their deposits. I would especially suggest a fresh examination of the fragments of older and igneous rocks met with in some of the drifts of the southern half of England, and hitherto too generally assumed to be of northern derivation. There are sources to

the south or south-east, from whence similar rock-fragments might have come, and from whence, indeed, they have found their way into the gravels that lie beneath the probable equivalents of our northern drift in France. The consideration is at least worthy of notice and inquiry, the more so since there are anomalies, some of them palæontological, which at present tend to make myself, I believe among others, inclined to object to the usually received notions. We are evidently on the eve of a revival of the study of what used to be called 'diluvial' deposits—one in which I trust our continental brethren will take more part than at present they seem inclined to. The older and firmer strata, rich in definite sections and fossil treasures, doubtless present greater attractions than the inconstant charms of gravel beds and sand pits, which, however, if perseveringly studied, are sure to yield their votaries abundant reward and ample results.

The gravels of Yorkshire and Nottinghamshire have been noticed by the Rev. W. Thorp, in a paper published in the Proceedings of the West Riding Geological and Polytechnic Society. He distinguishes three sets of gravels derived from different transporting currents, and notices the existence of considerable tracts that are quite bare. The first and most considerable of these gravels belongs to the northern drift, and contains fragments of rocks now *in situ* in Cumberland. It reaches considerable elevations; masses of granite from Shap Fells having passed over Stanmoor Forest at an altitude of 1400 feet, and over oolitic hills 1500 feet high, down to the east coast. The second range of gravel constitutes in one place a narrow tract, from one to two miles in breadth, touching the northern drift near the river Humber, and extending from Leeds by Ferrybridge to Goole. It contains pebbles derived almost wholly from sandstones of the coal districts of Yorkshire, mingled with fragments from the mountain limestone, and does not extend much north or south of the valleys of the Aire and Calder. Leeds stands upon it. The direction of the transporting current was from west to east. A similar east and west range extends from Doncaster to the south bank of the Humber, formed of coal sandstone pebbles, mingled with others from the mountain limestone of Derbyshire. He places other east and west gravels composed of magnesian limestone pebbles in the same category. The third set of gravels noticed by Mr. Thorp is peculiar to Nottinghamshire. It extends uninterruptedly southwards from Doncaster to the town of Nottingham, lying on the back of the new red sandstone, spreading in a thickness of from 3 to 8 feet, but not present on very abrupt hill sides. South of Nottingham and Derby it becomes intermingled with the northern drift, but in several places is capped by the magnesian limestone gravels of the second set. This third gravel contains no pebbles derived from the neighbouring strata, but is almost entirely made up of quartz fragments, smaller, more even, and more spherical than the boulders of the northern drift. Their drifting currents flowed north and south. Mr. Thorp maintains that this gravel constituted the ancient sea-bottom left by the waters which deposited the new red sandstone itself. The gravels of his second kind he believes to have been exported from the York-

shire coal-field during the epoch of emergence of the British area from the glacial sea; the waters being driven eastwards down the lines of valley which formed the course of easiest retreat. The absence of gravels in many districts he attributes to the protecting influence of high bluffs of land to the north of these bare areas.

The leading features of the northern drift in Yorkshire, as well as the other geological phænomena of the district, are sketched in a masterly style by Professor Phillips, in his lately published volume on the Mountains, Rivers, and Coasts of his native county. In the geological chapter of this work, the subject of the nomenclature of epochs is considered, and a scheme of terms suggested, founded chiefly on the leading organic characters of each section of time. A very neat and clear map of the geology of Yorkshire, by the same eminent observer, has been published during the year, and is remarkable for being printed by chromo-lithography, a process that is fast advancing to an astonishing degree of perfection. And here I may incidentally congratulate our science on the recent appointment of our illustrious associate to the Professorship of Geology in the University of Oxford; one that confers equal honour on the receiver and the givers. In this instance that famous school of learning has endeavoured earnestly and conscientiously to forward the true interests of science; and every geologist in the world will applaud the choice. A University that has boasted for ages of having held in especial honour our great master in Natural History, Aristotle, and that now possesses magnificent collections in all its departments, invaluable for study, may yet become a favoured home of Geology and Biology.

The Geology of Scotland has not received many descriptive contributions during the past year. One of the most interesting is the memoir on the Granitic district of Inverary, in Argyleshire, read before us by the Duke of Argyll. His Grace has rendered good service to the geology of his country before, for to him we owe the discovery of its older tertiary beds. In the paper he has now given, he deals with igneous and azoic rocks. The chief problem which he seeks to solve in the district under description, is the cause of the regular alternations of mica-slate and granite, the beds of which rocks lie conformable to each other at a considerable angle. After showing the insufficiency of any other mode of explanation, the noble author argues that the mica-slates, already completely consolidated and metamorphosed, fell in from a horizontal to an inclined position, and by falling forced the molten igneous matter between the loosened planes of stratification. The considerations put forward in this memoir are highly worthy of attention, and it is to be hoped that they will give rise to not a few minute examinations of the crystalline rocks of the Highlands, in localities where similar phænomena present themselves.

The Silurians of the south-west of Scotland have been described in detail, so far as Kirkcudbrightshire is concerned, by Professor Harkness, who considers these beds to represent successively the Llandeilo flags, the Caradoc sandstone, and the lower portion of the Upper

Silurians. The scantiness of palæontological evidence renders the exact determination of their equivalents peculiarly difficult.

In Scotland the subject of glacial phænomena continues to be discussed and investigated with unabated interest. Mr. Robert Chambers has been actively engaged in the collection of facts concerning the glaciation of Britain and the attendant phænomena. His views have been communicated at some length to the Royal Society of Edinburgh, and may be found printed at full in the fifty-fourth volume (for 1853) of the Edinburgh New Philosophical Journal. He recalls attention to the evidences of the presence of local systems of glaciers, of ordinary and typical constitution, in the mountain districts of North Wales, Cumbria, and Scotland, and notices fresh instances of this phænomenon. From it, however, he distinguishes the evidences of what he considers glacial action of a more general kind, manifested in Scotland in every part of the Highlands, and much of the Lowlands, in the rounding, smoothing, and striation of rocks, generally in the line of valley, and also in elevations to as much as 2000 feet above the sea-level. Professor Ramsay had previously demonstrated two distinct epochs of glaciers in North Wales. The direction of the icy agent in these cases Mr. Chambers maintains to have come from the north-west, and to have acted with little regard to the inequalities of the surface. He interprets the phænomena as indicating the passage over wide areas of an abrading agent, at the same time plastic and of volume sufficient to fill valleys several miles in breadth, and from one to two thousand feet in depth, and he maintains the probability of this agent being ice much water-charged and more mobile than as presented in an ordinary glacier. He holds the power of the denuding force of this agent to have been very considerable. The older boulder clay he regards as the detritus of this general glaciation, which he believes to have taken place at a period anterior to the epoch of the northern drift, which itself preceded the epoch of local glacier systems. There is much that is highly interesting and suggestive in Mr. Chambers' paper, even though we may not be inclined to go along with him unhesitatingly in his speculations. The subject of the ancient glacial phænomena of Britain, Scandinavia, and America, is evidently fast advancing towards new combinations, and the multiplication of local observations, of which many good examples are contributed by Mr. Chambers, will most effectually promote our progress towards definite conclusions. In the mean time those who occupy themselves with these inquiries should closely study the admirable and beautiful work on the existing glaciers of Norway, just contributed to science by Professor James D. Forbes of Edinburgh. The thorough knowledge and science of the author, his great experience, his searching and logical treatment of his subject, and the excellence of his style render all his works on this difficult matter models and guides.

The condition of the surface of the emerged land of the Scottish area during the epoch of general glaciation, the existence of which is inferred by Mr. Chambers, must have been very comparable with that noticed by Dr. Rink, in his late paper "On the Continental Ice

of Greenland *," and by Dr. Sutherland in his researches, published in our Journal, on the western coasts of Northern Greenland. In Greenland at the present moment we have a vast extent of land "covered," to use Dr. Rink's words, "with ice to a certain elevation; mountains and valleys levelled to an uniform plane; river-beds concealed, as well as every vestige of the original form of the country." The movement commencing far inland, which that able observer describes as thrusting the outward edge of this mass of ice forward towards the sea, would doubtless produce over a large area effects of general smoothing, grooving, and striation similar to those presented by the surface of Scotland. To every student of ancient glacial action, Dr. Rink's interesting paper must be of considerable value.

In Ireland the members of the Geological Society of Dublin have sent forth an interesting part of their Journal, containing the proceedings of the last session. Mr. Willson of the Geological Survey contributes an outline of his observations on the Geology of the Southern portion of the County of Cork, chiefly concerning the thickness of the rocks that intervene between the old red sandstone and the carboniferous limestone in that district. To some of the facts stated in this paper, I would direct attention for the sake of English investigators of the middle palæozoic strata. At Bally-cotten bay, shales, slates, grits, and flagstones alternate, and occupy the interval between the carboniferous limestone and Old Red, to the thickness of 2000 feet. At Monkstown similar beds are 2600 feet. More to the south, between the neighbourhood of Bandon and the Seven Heads, 3800 feet of strata were measured without reaching the limestone, and at the Seven Heads, the intermediate beds are 4500 feet, with no certainty of their uppermost portion being reached. Mr. J. Kelly, in an interesting paper "On the Quartz Rocks of the Northern Part of the County of Wicklow," combats the view adopted in some sections, published by the Geological Survey, to the effect that they are beds interstratified with slaty rocks, and maintains the amorphous character of the masses, and their intrusive origin. Considerable difficulties doubtless attend the certain delineation of the relations of these quartz rocks, in some measure owing to the state of the country, which is much obscured by drift. A compact and well-worked memoir on the Geology of Portraine, an isolated district in the neighbourhood of Dublin, famous for the interest of the Silurian fossils that have been procured from a small patch of strata of the Llandeilo type, contains the particulars of a highly interesting tract, previously undescribed in detail. The paper is by Mr. Henry Medlicott, a young geologist of varied accomplishments and much promise, who has lately gone out to India to join the Geological Survey under the direction of Mr. Oldham. Professor Haughton, of Trinity College, Dublin, commences a series of notes on the Irish mines, and, combining his eminent mathematical and physical acquirements with practical field geology, has read a memoir on the newer palæozoic rocks which border the Menai Straits. In this essay, after describing the physical structure

* Journal of the Royal Geographical Society, vol. xxiii. for 1853.

of the south-east side of the Menai, he enters into palæontological details, and connects his subject with the geology of Ireland, by showing at some length the analogy in lithological character and fossil contents, between the lower parts of the series of strata in question, and the 'yellow sandstone' of Mr. Griffith, as seen in the North of Ireland. He maintains that in this Welsh district no distinction exists between the Devonian and Carboniferous deposits, and that the entire series of beds, including the red sandstone conglomerates and yellow sandstones at its base, must be considered as a continuous whole. It must be borne in mind, however, by English geologists, that the so-called Irish Devonians alluded to belong to the neutral ground, between the typical Devonians and Carboniferous limestones, and that for many reasons their affinities may be regarded rather as appertaining to the latter than to the former palæozoic group.

A paper of a strictly Irish character, but bearing importantly on our own Silurian Geology, has been read by Messrs. Jukes and Wyley, on the structure of the northern part of the county of Wicklow. The authors show that the Lower Silurians rest unconformably on the edges of the Cambrian rocks of that locality, and that the granite does not bring up the Cambrian rocks on its flanks, but cuts up through the Silurian; the general dip to the rocks being towards the granite for a considerable space on each side of it.

The new edition of Mr. Griffith's beautiful map has this day been presented to us, enriched by many improvements. The Geological Survey of Ireland has completed the examination of the counties of Dublin, Wicklow, Wexford, Kildare, Carlow, and Waterford, and more than half of Kilkenny and Cork, with parts of the adjacent counties. All the observable data have been laid down on the six-inch maps, and the results published on the index maps of the five first counties. The sheet inch-map of Ireland having now been commenced, and four quarter-sheets, including the northern half of the county of Wicklow, &c., being nearly ready for publication, the early geological work of that portion has been revised and the lines laid down upon the new maps. The publication of these inch-maps may shortly be looked for, and it is to be hoped that the furtherance of this good work, by the aid of the Ordnance, will receive every encouragement from Government.

Geology of British Colonies and Possessions.

In noticing the progress that has been made during 1853 in this highly important branch of my theme, I shall confine myself almost entirely to remarks upon memoirs not contained in our own Journal. That publication is rich this year in contributions to colonial geology, essays of unquestionable value, and whose merits speak for themselves. Thus from the East we have received accounts of the researches in various parts of India of Dr. Andrew Fleming, Capt. Vieary, Mr. Frere, Lieut. Sankey, and Dr. Bell; and a notice of the geology of Labuan by Mr. Motley. Captain Nelson has given us the results of his researches among the coral

formations of the Bahamas, supplementary to those formerly communicated from the Bermudas by this distinguished officer. To Canadian geology Dr. Bigsby has added his account of the structure of the Quebec district. Mr. Dawson and Mr. Poole have added to our knowledge of the details of the Carboniferous formations of Nova Scotia. Dr. Sutherland has given us a full account of his observations in Baffin's Bay and among our inhospitable Arctic possessions, as well as notes on the neighbouring coasts of Greenland. Mr. Wathen has described the gold fields of Victoria; and to return to the Atlantic, Colonel Heneken has offered a contribution to the geology of the West Indies. On this last-named subject I would venture to offer a few remarks.

It is much to be desired that some able and active geologist, practised in the observation of the newer formations of Europe, would visit and explore the West Indian archipelago. There is no finer field for fresh research, and all that has hitherto been done, from the early labours of Sir Henry De la Beche in Jamaica, to the latest memoir, that of Colonel Heneken on San Domingo, communicated to the Society in March last, holds out a rich promise of reward to the man able and willing for the work. Colonel Heneken's account of San Domingo, with the accompanying palæontological comments by Mr. Moore and Mr. Lonsdale, is one of singular interest for the tertiary geologist and the inquirer into the geographical arrangements of the later epochs. The demonstration of something more than a relation of analogy between the fauna of the San Domingo and Panama tertiaries on the one hand, and that of the existing Panamian and Indo-Pacific regions on the other, is a significant advance, and points to an ancient disjunction between the masses of land in the North American area and those of the South, dating probably about the epoch of the middle tertiaries; whilst the indication of some identifications, even though few, of ground-living mollusks, not likely to enjoy a deep vertical range, with species living on the European side of the North Atlantic during the Miocene epoch, would seem to indicate an extensive stretch of land or of shallows from the West Indian region Europe-wards, that remarkably accords with some well-known indications afforded by the distribution of existing creatures.

The number of papers on East Indian geology, referred to as contained in our Journal, would of itself be ample evidence of the diligence and zeal of Indian geologists. But the student of the structure of the East must not confine his studies to the transactions of societies at home; in the journals of Indian Societies he will find many papers of great interest. The excellent report on the geological structure and mineral wealth of the salt range in the Punjaub, by Dr. Andrew Fleming, is an instance. It may be found in the Journal of the Asiatic Society of Bengal; in which work are some remarks by Capt. Young on the much-disputed subject of Laterite. The last-named paper contains interesting notices on the geology of Burmah. Dr. Kelaart, in another eastern periodical, has published his observations on the Laterite of Ceylon. The Journal of the Bombay branch of the Asiatic Society not unfrequently contains

essays of geological interest. Among these, in the last year, is one of the most valuable of recent additions to our knowledge of the geology of India, a memoir on the Geology of the Nagpur State, by the Rev. S. Hislop. A good deal had been done for the investigation of this interesting district, and in a paper communicated to the Society by Lieut. Sankey of the Madras Engineers, a detailed history was given of the results of the researches there of geologists and collectors, more especially Messrs. Hislop and Hunter, who had previously transmitted to us an extensive suite of fossils. Mr. Hislop maintains, supporting his opinion by forcible arguments, that the overlying trap of Central and Western India cannot have been poured out in the bed of the ocean, but must have been erupted in a lake or chain of lakes. The freshwater tertiaries that underlie the trap he considers to be of Eocene age, perhaps too positively, although his view is consistent with some indications afforded by the shells that occur in them; but it should be borne in mind by all describers of fluviatile and lacustrine formations, that mere analogy of form is a very bad guide in the determination of the epoch of freshwater mollusks. "The sandstone of Central India, which appears to be identical with the diamond sandstone of Southern India, belongs with its associated shale and the Indian Coal-measures to the Lower Oolitic formation." He suggests the probability of these sandstones being of freshwater origin, and maintains that the Deccan exhibits no evidence of having been submerged by the ocean since a period anterior to the Oolite.

The description of the fossil animals of the nummulitic rocks of India, by Vicomte d'Archiac and Jules Haime, elsewhere alluded to when the monograph of Nummulites was mentioned, will, when completed, form a manual of the highest value for the study of this extensive formation in the East. The part already published contains the account of the Corals and Echinoderms (as well as the Nummulites), and is preceded by a review of the geology of the nummulitic region of India. This chapter is not a mere summary of what had previously been known and published. It contains much that is new, facts of high value derived from the researches of our associates, Vicary, A. Fleming, Oldham, R. Strachey, Thomson, and J. D. Hooker. Sir Roderick Murchison has been the means of placing these fresh data at the disposal of M. d'Archiac. The result of these studies has been the confirmation of the complete independence of the nummulitic in regard to the Cretaceous formations. "In the province of Cutch, in Seinde, Beloochistan, the Punjaub, and along the slopes of the Himalaya," remarks M. d'Archiac, "the beds beneath the nummulitic limestones exhibit nowhere the characters of any stage whatsoever of the chalk, whilst, wherever the substratum has been recognized, it exhibits those of carbonaceous deposits with clays and sandstones belonging to the lower tertiary formation, and resting either on Jurassic strata, or on more ancient rocks of which the age is yet unsettled."

Every student of Indian geology will be delighted at the appearance of the 'Himalayan Journals' of Dr. Joseph Hooker, a work that will do much to sustain the reputation of the British school of

Natural History. The geologist will find in it a rich store of facts of the highest interest, and for the inquirer into glacial phænomena it abounds with new and invaluable data. I may say the same for the geographical memoir of Capt. Richard Strachey on Western Thibet; and we all look forward anxiously to the publication of the detailed account, now in progress, of his geological researches, knowing as we do, how rich a store of new and important facts was accumulated during his adventurous journeys.

It is most gratifying to think that at the present moment no small proportion of our foreign possessions is being surveyed geologically by able and trained observers, and that the vague and often incomprehensible statements of self-satisfied and shrewd though ignorant miners and unqualified travellers, are fast being supplanted by the results of careful and accurate research. In Canada Mr. Logan pursues his great work, with able assistants, as zealously and successfully as during former years. In Eastern India Mr. Oldham is similarly engaged, and has gathered around him several young geologists of promise, trained in the methods of European research. In the provinces of Australia, mineral exploration is not abandoned to mere gold-seekers, and the government reports are now scientific documents. Mr. Stutchbury and Mr. Clark are at work in New South Wales, and Mr. Selwyn has undertaken the exploration of Victoria, with the advantage of having previously passed through a strict geological discipline in the survey of North Wales. In our colony of the Cape of Good Hope a geological survey is being regularly conducted by Mr. Geddes Bain, whose private researches had previously produced remarkable discoveries; and in that of Natal, a similar official exploration is, I believe, about to be conducted by the Surveyor-general, Dr. Stanger, whose perfect qualifications for the office are well-known to many Members of our Society.

Progress of Geology abroad.

Of late years the literature of our science has annually received so enormous an increase, that to keep pace with the progress of the details of local geological research is a labour almost beyond the ability of a single individual. In every civilized country the number of pursuers of geology is rapidly multiplying, and the transactions of foreign societies have become prolific in memoirs treating on all departments of our science. France, Prussia, Austria, Russia, Italy, and Scandinavia have all, during the past year, contributed largely through their geological and other societies as well as by separate treatises. In the New World a like manifestation of scientific activity is exhibited, so it would be presumptuous to pretend to report, in an address of reasonable dimensions, the particulars of geological progress abroad during 1853. I shall therefore confine myself almost entirely to the noticing of a few papers and works upon foreign geology that bear more or less directly upon questions of peculiar interest to workers at home. One of the most important, the great work of Barrande, I have already noticed at some length. The date

(1852) of the excellent and truly valuable works of Dale Owen (on Wisconsin, &c.) and James Hall (Palæontology of New York), places them out of my plan; both are of the highest merit, and ought to be carefully perused by every British student of palæozoic formations. The beautiful and laborious map of Belgium by Dumont, one of the great works of our day, has only just come to hand in England; as likewise have the very welcome second volume of Professor Studer's 'Geology of Switzerland,' and the commencement of a national work on the geological survey of the Netherlands.

The admirable 'History of the Progress of Geology from 1834 to 1852,' by Vicomte d'Archiac, a work that does equal honour to the Geological Society of France and the French government, under whose auspices it is published, continues to advance steadily, though still far from its completion. During 1853 the second part of the fifth volume has been issued. This part is devoted, like the last, to the Cretaceous formations; the regions treated of being the Iberian peninsula, Italy, the countries around the Levant, those around the Baltic, the north-west and centre of Germany, Poland, Gallicia, the Carpathians, Russia, Asia, Africa, and America. A prefatory chapter is occupied with the discussion of some general principles involved in the consideration of the phænomena afterwards described, and in a concluding essay the author reviews the geographical and stratigraphical distribution of the Cretaceous formation considered as a whole. The essential characters of this work are such as to preclude any analysis in an Address of this kind. I feel bound, however, as one of the many who feel grateful to M. d'Archiac for the inappreciable assistance afforded by this labour of love on his part, to bear the strongest testimony to the ability, learning, philosophical spirit, and impartiality with which he has executed so far the difficult and delicate task undertaken by him in the composition of this History.

The accession to our ranks of a new and able observer is always a subject of commemoration and congratulation; still more so ought it to be, when our gain is in some partially-explored region, and one where men of science are few and far between. We can boast of such an accession in Senhor Carlos Ribeiro, whose excellent notices of the Carboniferous and Silurian formations of the neighbourhood of Busaço in Portugal have been communicated to us in an ably-condensed memoir by Mr. Sharpe, with notes of high value from several of our Members. We may hope that this paper, a valuable addition to the series of contributions to the geology of Portugal, mostly due to the personal labours of our Treasurer, is a precursor of a full and detailed exploration, by native observers, of a region in great part as yet virgin ground for our science.

The geological structure of a large portion of Spain has been outlined in masterly style by MM. de Verneuil and Collomb in a memoir entitled "Coup d'œil sur la Constitution Géologique de plusieurs Provinces de l'Espagne," communicated to the Geological Society of France. To every scientific traveller visiting the Peninsula hereafter this excellent treatise will be indispensable. It is illustrated by sections and figures of organic remains; a geological map of Spain is

promised. The authors, ever ready to acknowledge and do justice to the labours of those who have gone before them, not only satisfactorily show that the spirit of geological research is active and working among Spanish men of science, but also demonstrate, by a detailed catalogue of no fewer than 154 works and memoirs, that Spanish geology is not so unexplored as many of us are apt to fancy. Among the names of British contributors in his long list are those of Lyell, Silverton, Cook, Traill, Daubeny, Pratt, and Smith of Jordan Hill.

Although when British geologists make raids into the neighbouring regions of the Continent, especially France, Belgium, and Germany, where so many able and eminent brethren of the hammer are exploring with success the structure of their native countries, the object and purpose of their predatory incursions are chiefly to benefit by the experience and teaching of their scientific neighbours, occasionally they feel bound to differ and attempt to correct. There are some provinces, indeed, so closely allied in their geological constitution to well-explored portions of our own archipelago that they seem as outliers of our own geology, and therefore fair fields for critical inquiry. One of these lies almost beside our shores, and is well worthy of examination and study by every geologist engaged in the examination of the upper and middle palæozoics. I allude to the country about Marquise in the neighbourhood of Boulogne. Although not unfrequently described by both French and English observers, much obscurity hangs over the chronological affinities of the palæozoic rocks of this district; and although latterly the demonstration of these relations was being more nearly approached than a few years ago, there still remained much to be done, and none among our countrymen is fitter to undertake their exploration and elucidation than Mr. Austen, whose knowledge of the palæozoics of Devon peculiarly qualify him for this task. The valuable memoir communicated to us in March last is the account of his researches. Leaving its details, as published in our Journal, to explain themselves, I will merely call attention to two or three results of leading interest. Mr. Austen clearly proves that all the Palæozoics of this district belong to the Carboniferous and Devonian series. If any doubt could be entertained respecting any portion of these beds, it would fall upon the black schist of Caffiers, the lowest visible member, and hitherto regarded as unquestionably Silurian; this he sets provisionally aside. But Mr Sharpe, in his excellent appended note, places the supposed lower palæozoic nature of this schist in an extremely doubtful position, by showing that the so-called Graptolites contained in it are really plants. The determination of the true relations of the yellow sandstone belts, with their characteristic contained bivalves, is an important step, and gives us a zone of division between the carboniferous and Devonian limestones, the true equivalents of which are evident and similar in our own regions of Devon and in Ireland. In the latter country this horizon marks distinctly what may be regarded as the line of division between the lower carboniferous rocks—the carboniferous slates, &c. of Dr. Griffith—and the upper portion of what may be considered the Devonian series

proper, that part in which the Kilkenny beds, containing *Cyclopteris hibernicus*, *Anodon Jukesii*, and other fossils indicative of shallow and probably estuary or fluviatile conditions, occur. The Marwood sandstones of North Devon (and the Pilton and Petherwin groups) correspond with it. To what extent the comparison of the beds at Marquise lying below this band, the Ferques and Fiennes groups, and the underlying beds are to be regarded as representing the whole Devonian series, is a question about which Mr. Austen and Mr. Sharpe are at variance, though the much-wished-for publication of M. Bouchard's lists of Ferques fossils would possibly go far towards settling the matter in Mr. Austen's favour, his view being that the Eifelian, Ahrian, and Rhenane series of Dumont are here represented. These are points that time will certainly clear up. In the meanwhile a good stride onwards has been made towards assisting our own students of Devonians, who would do well to study carefully this memoir upon the Marquise beds, and to ponder over the excellent and highly suggestive comparative table of the succession of mineral characters and physical conditions that is appended to it.

The Devonians of the Rhenish provinces have lately received a full share of attention, chiefly, however, in a palæontological point of view. I shall have hereafter to call attention to several memoirs bearing upon their organic history. A fresh geological description of the Eifel has been published by the veteran Steininger, illustrated by sections and figures of new fossils. It is very questionable, however, how far the identification of Devonian with Siluriau species, put forth in this work, can be accepted. The author's mistakes have arisen chiefly from his retaining the old but now untenable notion of the Silurian place of the German Spirifer-Sandstones. The most valuable contributions to the elucidation of the German Devonians are the works of the brothers Sandberger, who have done much towards the definition and precise classification of this series of rock-formations.

The problem of the true relations of the Calcaire pisolitique of the Paris basin is likely before long to receive some satisfactory solution. Hitherto the balance of opinion has inclined in the direction of the view so ably advocated by M. d'Archiac, and urged also by Sir Charles Lyell, to the effect that the fauna of this formatiou and its stratigraphical relations warrant the reference of it to the tertiary series. M. Hébert, on the other hand, and with much show of reason, insists not only on its connection with the cretaceous series, but also of its equivalence to the yellow chalk of Maestricht. This question is becoming one of general interest, and has already had its influence in debated portions of our own geology. The views of M. Hébert are stated at some length in a note on the synchronism of the Calcaire pisolitique of the environs of Paris and the Upper Chalk of Maestricht, published in the Bulletin of the Royal Academy of Belgium. The English geologists must bear in mind that the term "Upper Chalk" thus used by M. Hébert, and proposed by him as a denomination for that highest portion of the cretaceous series in which he would place as synchronous the Calcaire pisolitique, the Faxoe chalk or

Terrain Danien, and the baeulite limestoue of the Cotentin, is not to be understood as embraeing the beds which we are aeeustomed to eall "Upper Chalk" in England, and which are especially developed in Norfolk and the east of Kent. The equivalents of the latter, of which the *Cardiaster granulosus* may be mentioned as a characteristic, widely diffused, and guiding fossil, may be seen at Ciplcy and near Maestricht underlying the yellow ehalk with *Hemipneustes radiatus*, i. e., the "Craie supérieure" of Hébert. I have never seen in England any beds whieh could satisfactorily be assigned to the last-meutioned series, but think it extremely probable that the ehalk of Antrim in Ireland, which assuredly should be regarded in its greater part as equivalent to our English Upper or Norwieh ehalk, will be fouud to inelude equivalents of the Maestrieht or yellow chalk of the eontinent. I make this suggestion in consequenee of having carefully examined the fine eollection of Irish fossils brought together and first deseribed by Colonel Portlock, and the still more extensive suite in the Belfast Museum eollected by Mr. MacAdam, for the publication of whose long-eontiuued labours among the formations of the North-west of Ireland, all geologists aequainted with that able observer's perseverauce and careful inquiries, now eontinued over many years, impatiently await.

Mr. Prestwieh has communicated to the Geological Society of Franee his views respeeting the position of the tertiary sands and laeustrine limestone of Rilly (Marne). The true plaee of these beds in the series of lower sands of the Paris basin had not been determined with eertainty. Mueh general interest attaches to the question, since, if, as has been maintained by some eminent French geologists, the freshwater limestone of Rilly is more aneient than any known tertiary deposit (providing the referenee of the Calcaire pisolitique to the Cretaeeous group, as proposed by M. Hébert, be aeeepted), then we have elear proof of the entering in of the Tertiary epoch in the area under dispute with terrestrial and fluviatile or laeustrine eonditions; the Rilly limestone in this case having been deposited in lakes upon the emerged Cretaeeous surfaee. Mr. Prestwieh maintains, however, the iudependenee of the sands and the limestones, and the superposition of the latter on the former. He, for the first time, records the presenee of fossils in these sands, apparently mueh in the same eondition as they appear in the similar, though not homologous Headon sands in the Isle of Wight. As in the latter case, they are marine. He holds these sands to belong to the same deposits with those of Chalons-sur-Vesle and Chenay, both marine sands below the lignites. He eoncludes that the Rilly limestone was preeeded by a marine deposit of tertiary age, and was not the most aneient of the tertiaries. On many and good grounds, he maintains that it was a loeal travertine formed in a small lake, swamp, or marsh; a view supported by the fact that out of forty-five speeies of Rilly shells, no fewer than thirty are of terrestrial habits, whilst most of those that are aquatic are pulmoniferous types. The presence of *Aviculæ* in these beds would seem to indieate the neighbourhood of salt water.

The treatise on the Tertiaries of the Mayence basin, by the brothers Sandberger, is for all who study the relations of the middle to the lower tertiaries, one of the most valuable contributions to our science during the year. Since, however, we are shortly to have presented to us the results of Mr. Hamilton's prolonged and careful labours in the same region, I shall abstain for the present from any comments on an essay of peculiar interest to myself, as well as on other recent German papers, especially those by Beyrich and Dunker, affecting the same, or closely allied localities.

Organic Remains.

The enormous increase of palæontological observations may be measured by a comparison between the number of species recorded in the first edition of Professor Morris's Catalogue of British fossils, and the number mentioned in those portions of the new edition that have gone through the press, and will shortly be published.

The number of plants recorded in 1843 was 510; in 1853, 652 are cited. The increase is chiefly among Mesozoic and Tertiary types. A great deal has been done to elucidate the structure and affinities of fossil plants in the interval, especially by Dr. Hooker, Mr. Charles Bunbury, Prof. King, Mr. Dawes, and Mr. Binney, but not so much towards adding new names to our lists of species. In Fossil Botany this course of proceeding is a sign of advance of knowledge. The most marked increase in number of recorded species is among the oolitic and Wealden beds. The late lamented Dr. Mantell did much of late years towards increasing the latter list. Were all the known fragments of distinct vegetables found in our tertiaries monographed and named in the manner of those I shall have presently to mention, described and figured in the lately published memoirs by Austrian botanists, our lists would be considerably increased. They certainly ought to be made the subject of a treatise, and might be advantageously taken up by the Palæontographical Society, which, as yet, has given us no separate memoir on British fossil plants.

The Amorphozoa come next. In 1843, 76 named forms were recorded. In 1853, the number is increased to 116. The increase is in a great measure due to the labours of Mr. Toulmin Smith among the Ventriculidæ, which, notwithstanding the arguments of their investigator in advocacy of their Polyzoan, and consequently Mollusean origin, naturalists are generally of accord to keep in their old place beside the Sponges.

The Foraminifera, 82 of which are mentioned as named types in the list of 1843, have increased to 168, besides numerous indications of unpublished and, as yet, unnamed forms. The next ten years will probably triple the amount of named fossil species of these exquisite minims of creation. The additions are chiefly new identifications of British fossils, with species described by continental authors, especially by Alcide d'Orbigny and Reuss. The merit of determining these is, I believe, in great part due to our Assistant-secretary, Mr. Rupert Jones, whose authority stands very high in all departments of microscopic palæozoology. Mr. Jones

himself is an addition to the list of British Palæontologists during the last ten years, and one we all welcome. The labours of Dr. Williamson and Dr. Carpenter have also done much towards clearing up our fossil Foraminifera; and the untiring exertions of Mr. Harris, of Charing, though inconspicuous in print, have, I believe, been a chief source of fresh materials towards the history of our cretaceous species.

In the first edition, the Zoophyta are combined with the Bryozoa. When the latter are eliminated there remain 183 zoophytes, chiefly corals. This number has been prodigiously added to within the last ten years, no fewer than 438 species being enumerated in the new catalogue. The increase, in this instance, is due to an entirely new treatment of the subject. To Milne-Edwards and Jules Haime a large proportion of the additions are indebted for their place. Mr. Lonsdale and Professor M'Coy have also contributed extensively.

The Bryozoa, a few years ago regarded as Zoophytes, but now known to be low forms of the subkingdom Mollusca, amounted to about 132 in the first edition. In the new catalogue, they constitute a roll of no fewer than 249 species. This extended list is due to many investigations, and the newly-recorded types come from formations of all ages. Attention seems to have been suddenly directed to these curious bodies both at home and abroad. The study of the British fossil species, vast as is the increase of the recorded numbers, can be regarded only as in its commencement. I trust that geologists who may direct their attention to these bodies hereafter, will bear in mind the complete and searching analysis of the existing species drawn up by Mr. Busk for the British Museum, and guide themselves in describing the fossils by the example of that valuable treatise.

The Echinodermata, 266 in number in 1843, are now 479; the record of species is daily increasing, but I do not think likely eventually to extend beyond 500 British forms. Major Austin, Professor M'Coy, Dr. Wright, and myself have been the principal workers in this beautiful, and in a geological point of view, invaluable order. The additions of the entire family, including not a few genera and species, of Cystidea to the list, (for the Sphæronites of the former catalogue is probably not a cystidean,) a group as characteristic of the lower palæozoic formation, as the Graptolites or myriads of Trilobites are, is one of the most striking instances of the progress of palæontological research, and one due for several of its most curious facts to the exertions of Mr. Fletcher, and Mr. John Gray of Dudley.

The named Annelida were 79 in 1843, they are now 129. The most interesting additions are among palæozoic forms.

The Cirripedia, 21 in 1843, are now 42. The value of the increase in this instance is not to be estimated by the merely doubling of the number. They have been thoroughly sifted by a master-hand, analysed with incomparable care, and by a combination of unsurpassed labour with judgement and knowledge of the highest kind, have been brought to a state which may be regarded as, at least for many years to come, the epoch of maximum in their investigation. To Charles Darwin we are indebted for this service.

The Crustacea are now 291; in 1843, they were 159. This is an enormous advance, and curious, since in great part it has arisen from additions to the list of palæozoic species. It marks, moreover, not merely an advance of names, but one of knowledge, as may be judged from an inspection of the changes in the generic list. The Trilobites have undergone a complete revision, and the number of species of those singular animals is vastly increased, thanks more especially to the work done by Salter and by M'Coy. The Cytheridæ and Cypridida have become a feature in the catalogue, mainly in consequence of the researches of Rupert Jones. Professor M'Coy has largely added also to the list of these tribes, and to the catalogue of the higher crustacea from our mesozoic and lower tertiary strata.

The additions to the list of fossil insects more than double this portion of the catalogue. They are due to the Rev. P. Brodie, and are entirely derived from mesozoic strata, chiefly from the Purbecks and Lias. In this department there is a considerable amount of unpublished materials existing in collections.

The number of Brachiopoda has swollen from 459 to 668, an addition of more than 200 species! In the mean time they have been undergoing complete and thorough revision. Mr. Davidson, whose appearance among us as a British palæontologist has taken place in the interval between the two editions, is foremost among the workers in this department, one greatly increased also by the labours of King, M'Coy, and Salter. Some very interesting contributions have come from Mr. D. Sharpe, and Mr. C. Moore of Ilminster. The important discovery of Liassic species of Leptæna and Thecidium in Britain is due to the last-named observer.

The catalogue included 318 Monomyarian Bivalves in 1843; in the new edition 577 are recorded. The additions in this instance come from numerous sources. Both in this and the following group we owe much to the labours of Mr. Morris and Mr. Lycett among the Oolites.

The lists of the remaining groups of fossil animals will, when completed, show a comparable increase in almost every section. In the highest, a large accession as well as a revision of species, will give a new value to the catalogue; many of the researches of Professor Owen among the reptiles and the warm-blooded Vertebrata, and of Sir Philip Egerton among the Fishes, having been given to the world in the interval. Indeed, scarcely a month now passes without the appearance of some published contribution to British palæontology.

The volume, or rather fasciculus of volumes, for the year 1853 issued by the Palæontographical Society is, in respect of richness of illustration and value of matter, one of the finest productions of this useful union. In its distinctive features it differs somewhat from its predecessors, inasmuch as a considerable portion of it is occupied by a series of elaborate treatises on the anatomy, microscopic structure, and systematic arrangement of the Brachiopoda, respectively contributed by Professor Owen, Dr. Carpenter, and Mr. Davidson. The anatomical plates attached to this memoir are, without exception, the most beautiful engravings that ever illuminated a natural-

history treatise. The lithographic plates illustrative of the genera of Brachiopoda are also excellent specimens of their kind, both in execution and arrangement. They are remarkable, not only for their fidelity, but also for their artistic merits, the more so since they are the work of an amateur in art, our accomplished associate, Mr. Davidson, to whose generosity and zeal for science, the world of geologists is deeply indebted for these admirable drawings. The former annual volumes issued by the Palæontographical Society have scarcely, owing to accidental circumstances, received notice in the Anniversary addresses, and I take this opportunity of offering a word of congratulation to the geologists of Great Britain, on the services rendered to their science through this remarkable series of monographs,—products of disinterested zeal and earnest co-operation.

When the Palæontographical Society was started, its founders could not have anticipated the success that has crowned its exertions, or the facility with which able and enthusiastic labourers in the field of science it proposed to cultivate, would have been found willing to devote their gratuitous exertions to the work. Hence there has arisen an inequality of plan and difference of treatment in the several monographs published annually, much to be regretted, but scarcely now to be remedied. Where a subject mainly of importance to a branch of zoology that chiefly concerns the geologist, such as that of the Brachiopodous Mollusks, is treated fully and completely as in the volume for 1853, we are grateful for the boon, even though such a treatise was foreign to the original intentions of the Society. But in the majority of instances it is doubtful how far it is desirable to expend the resources of the Society on printing purely zoological matter mixed up with the palæontological descriptions, and necessarily imperfect, and causing much repetition. Good figures and good descriptions of fossils are the true ends to be kept in view. It is also to be regretted that the Society had not started with a definite scheme of monographs; each and all should have been either stratigraphical, or else systematic; but we have now a mixture of both, which sooner or later will be the cause of not a little clashing and confusion.

The more strictly palæontographical portion of this volume is occupied by the continuations of the Monographs of British Fossil Corals, by Milne-Edwards and Jules Haime; of Mollusca of the Great Oolite, by Morris and Lycett; of the Crag Mollusca, by Searles Wood; and of the British Fossil Reptilia, by Owen. About 250 species of fossils of various orders are described and figured. Of these about two-fifths are either wholly new to science, or else new to the British Fossil Fauna—no small addition to our knowledge of extinct animals to come from one Society in a year.

In this part of the monograph of the Corals our Devonian species are described and figured, British examples only being selected for delineation, a precaution in a work of this character that cannot be too strictly attended to, since on its essentially local or topographical features much of the peculiar interest and value attached to it must depend. Sad mistakes in other works have been committed through the neglect of this precaution by more authors than one, and many a

dispute might have been prevented, had the exact locality, or if none was known for certain, the fact of the want of knowledge, of the specimen figured, been precisely stated. Forty-six corals occur in the British Devonian strata, being rather less than one-third of all known Devonian corals. Almost half of these are as yet peculiarly British, and of the others only six (five of them being also continental in Europe) occur on the other side of the Atlantic, a fact which, when we call to mind the wide latitudinal range of the Anthozoa, has an important significance in its bearing upon the determination of the geography of the northern hemisphere during the Devonian epoch. Three only of our Devonian corals are regarded by Milne-Edwards and Jules Haime as identical with Silurian species, whilst they consider all the others as peculiar to their epoch. All the species described belong to the groups *Zoantharia tabulata* and *Zoantharia rugosa*, and the most conspicuous and recent-looking corals of the Devonian reefs and banks were members of the latter suborder, one of which there are no living representatives. Hence all inductions drawn from the presence and forms of these zoophytes respecting the prevalence of a warm or tropical climate within our area during the epoch of their flourishing must be set aside, since they have been founded on the mistaking of analogies for affinities. If we accept the views promulgated concerning the structure and classification of corals by Milne-Edwards and Haime—numerous facts in whose support are accumulated in the several parts of this monograph—the prevailing opinions concerning the physical condition of the palæozoic epochs must be very considerably modified or subdued, and the separation of those vast and infinitely remote periods from the stages in time that succeeded them be made even more manifest than was indicated by the phænomena presented by other groups of palæozoic creatures.

The appearance of the first part of the "Description of the Fossil remains of Mollusca found in the Chalk of England," by our valued Treasurer, Mr. Daniel Sharpe, will be hailed with pleasure by students of cretaceous beds all over Europe. The portion published embraces the Belemnites, the Nautili, and part of the Ammonites, and contains descriptions and figures of 24 species, of which two are wholly new, and six new to British lists. The range of cretaceous strata from whence the specimens described have been procured, extends from the Upper Chalk of Norfolk and Gravesend, to the Chloritic marl of the Isle of Wight, and "Chalk with green grains" of Somersetshire. It is worthy of notice that of the Nautili described, several are recorded as having an extensive vertical distribution; thus *Nautilus lævigatus* ranges from the Upper Greensand to the Upper Chalk, whilst *Nautilus pseudo-elegans*, *N. radiatus*, *N. neocomiensis*, and *N. undulatus* occur in both the upper and lower divisions of the Cretaceous system; in others words, both above and below the Gault. Every fact of this kind well ascertained, is of no small interest at present, when there is an extreme and unwholesome tendency on the part of many palæontologists to insist *à priori* upon the distinctness of species coming from different stages, and to force their diagnoses accordingly.

The part of the "Monograph of the Mollusca from the Great Oolite," issued for 1853, is devoted to the Lamellibranchiate Bivalves (not yet completed), of which 116 species are here described and figured. Of these 36 are new to science, and 24 continental forms new to British lists. The authors remark that a large proportion of the Oolitic Lamellibranchiata had shells whose hinges were either a lengthened hinge-plate with a parallel series of transverse or oblique teeth, *i. e.* a hinge of the Arcoid type; or a toothless hinge of the Mytiloid and Myoid types; or a hinge with a ligamentary fossa only (as *Pecten* and its allies); or with the ligament inserted in distinct pits (*Perna* and its allies). Shells with cardinal teeth constituted only a minority: hence the *Veneroid* forms of the oolites are especially few in number. The preceding parts of this valuable monograph were equally rich in facts of a general character, and consequently now so well known that they need not be recalled here.

Professor Owen's instalment for 1853 towards his great monograph of the British Fossil Reptiles includes the Chelonia Paludinosa of the Wealden and Purbeck beds. Eight species are described and figured (six of them new), members of the genera *Pleurosternon*, *Chelone*, and *Platemys*. Those of the first named genus (four species) are all from the Purbecks, those of the two latter from the Wealden beds, properly so called. So far, then, the Reptilia tend to support the views that I have promulgated, after a careful and extended study of the Dorsetshire Purbecks and Wealdens, to the effect that these groups are not members of one series of freshwater beds, but perfectly distinct, and indeed belonging in part (the Purbecks) to the Lower, and in part (the Wealdens) to the Upper Mesozoic epoch. During the past summer I have had occasion again to go over the sections in the Isle of Purbeck, deliberately and in minute detail, and I remain confidently of the opinion which I put forth at the Edinburgh Meeting of the British Association in 1850. The detailed memoir on this subject, to be amply illustrated, is in progress, and if possible will be published in the course of the present year. In the mean time I do not regret the delay, since I have thereby been enabled to work out deliberately numerous points requiring time for their elucidation.

The monograph of the Crag Mollusca is fast advancing towards completion, and the fresh part is as remarkable as the former ones for the fulness of knowledge of the subject that has throughout characterized this important contribution to British Palæontology. Mr. Wood has spared no pains, and has worked from the most ample materials. The genera of Bivalves with cardinal teeth occupy this portion of his treatise. Of these 57 species are enumerated; not a few are now fully described and figured as British fossils for the first time, the previous notices of them having been restricted to a bare mention. I would earnestly urge upon continental geologists the consideration of the results at which Mr. Searles Wood has arrived in this most careful monograph. There cannot be a doubt that the epoch (or rather epochs) of the Crag, was as distinct from that of the

present stage in the world's history,—whether we consider the physical conditions of the area from whence our data are derived, either under a climatal or an orographical aspect, or the natural-history features of the population of that area,—as any "étage" of the upper or lower Mesozoic period was from any proximate "étage." Nay, the difference was even greater; for the physical and natural-history characters of the Pleistocene epoch that intervened were quite of as much differential importance as those of either "étage." Yet now that we know the Crag Mollusean fauna, we might almost say perfectly, no sane naturalist can for a moment deny that a large proportion of the species are positively identical with living types. Let those who would hastily draw a line of death between the faunas of proximate "étages," and regulate their geological conclusions accordingly, ponder well over this significant fact.

The essay on the classification of the Brachiopoda, by Mr. Davidson, contains the conclusions arrived at after many years of conscientious labour, mainly devoted to this interesting order of Mollusks, for whose illustration we owe so much to his pen and pencil. No other palæontologist has ever had so great an amount of perfect materials for his particular task at his command, and neither expense nor labour has been spared by our indefatigable associate to render his monograph as perfect as possible. If any of our brethren dissent from some of his specific decisions, they must all admit that they have been arrived at on no superficial grounds. The portion of Mr. Davidson's work now sent forth is entirely systematic, and is devoted chiefly to an exposition of the characters and definition of the families and genera of Brachiopoda. He admits 33 genera assembled under 10 principal families, with some intermediate and doubtful or provisional groups. As he has endeavoured to define his genera on the strictest natural characters, and appears to have succeeded in arriving at an arrangement, in the main sound and near to the truth, it becomes an inquiry of considerable interest to ascertain how far the ranges of these genera are continuous in time; in other words, whether the theory of unique generic time-areas be borne out among the Brachiopoda, now that we may be said to have attained so extensive a knowledge of their generic and specific types. This was doubtless the idea working in the mind of Von Buch, when with indifferent materials, he attempted to fix the characters of the fossil Brachiopoda, and plainly has often influenced the numerous attempts at their classification made by subsequent palæontologists. I have no reason to suppose that an *à priori* hypothesis, connected with either time- or space-distribution, influenced Mr. Davidson in coming to his final arrangement, and therefore I have been the more curious to see how far that arrangement accorded with geological considerations.

In the first family, *Terebratulidæ*, the typical genus *Terebratula* (of which *Terebratulina* and *Waldheimia* are regarded as subgenera), the succession of types is continuous from the middle palæozoic or Devonian epoch to the present time; whilst the other genera are either Upper Mesozoic, Tertiary, and recent (as *Terebratella* and

Argiope), exclusively Upper Mesozoic (as *Magas*), or exclusively recent (as *Bouchardia*, *Kraussia*, and *Morrisia*).

Stringocephalus follows as the type of a provisional family, exclusively Devonian.

The *Thecididæ*, represented by *Thecidium* alone, range continuously from the Trias to the present time.

The *Spiriferidæ* concentrate towards the palæozoic pole. In this family Mr. Davidson includes *Spirifer* (with *Spiriferina* and *Cyrtia* as sections), *Athyris*, *Spirigera*, *Uncites* and *Atrypa*.

Rhynchonella, with *Camerophoria* and *Pentamerus*, form a family under the name of *Rhynchonellidæ*. The absence of perforations in the shell is the rule in this group. The typical genus is one of the links between the palæozoic and present epoch, and has its maximum in the Mesozoic.

The *Strophomenidæ*, *Productidæ*, and *Calceolidæ* all concentrate in the Palæozoics; *Leptæna* only, in the first named family, extending into the lower Jurassic strata.

In the *Craniadæ*, represented by the single genus *Crania*, we have a type of Brachiopod almost equally present at all epochs. The nearly allied group of *Discinidæ*, though extending to the present, is generically concentrated in the Lower Palæozoics. The same remark may be made respecting the *Lingulidæ*.

Accepting the genera adopted by Mr. Davidson as mutually equivalent groups, and regarding their distribution in time as determined by him from a vast amount of specific materials, enough to induce us to believe that future discoveries will not materially disturb any inferences drawn from the numbers as now presented to us, then we arrive at several striking conclusions concerning the entire sub-class. Regarding the Present and the Lower Palæozoic epochs as opposite poles of time, we find the generic types among the Brachiopoda concentrate as it were around or towards each, whilst they depauperate towards the equatorial region of the scheme, about which indeed no generic types originate. The loop-armed types are regnant, as it were, anteally, the spiral-armed types posteally; and the latter are in the main so dominant, that the Brachiopoda, as a great assemblage of types, has its major development towards the past, its minor towards the present, and its zero in the parting epoch between the palæozoic and after-ages.

Some special memoirs on fossil Brachiopoda may here be noticed. Mr. Davidson has communicated an excellent tabular view of the classification and distribution of the genera to the 'Bulletin' of the Geological Society of France; and in our own Journal he has described and figured a number of species from the Devonian rocks of China, a region where some future palæontologist is likely to reap a rich palæozoic harvest. The remarkable discoveries in the lias of our own country by Mr. Moore of Ilminster, have found a parallel in France, where M. Eugene Eudes-Deslongchamps, who promises to be worthy of the distinguished name of his father, has found numerous species of *Leptæna* and *Thecidea* in the Liassac beds of several localities in Calvados. His essay, amply illustrated, forms part of the newly

published volume (the ninth) of the Mémoires de la Soeiété Linnéenne de Normandie, a work in whieh not a few geologieal notiees of interest may be found. It is indeed remarkable that the two genera in question, the one until lately regarded as eharaeteristieally and peeuliarly palæozoie, and the other as prineipally eretaeeous, should have their epoehs of eessation and eommeneement thus as it were in eontaet.

One of the distinetive features of our seienee duriug the year just past, is the monograph of Nummulites by Vieomte d'Arehiae, eonstituting a portion of the "Deseription des Animanx Fossiles du Groupe Numnulitique de l'Inde." For some time geologists have looked forward anxiously to the appearanee of this treatise, the fruit of eareful and eonseientious researehes, eondueted amid abundant materials, and guided by the wise, logieal and truth-seeking spirit, so eharaeteristie of its illustrious author. They have not been disappointed; the result of his labours is the produetion of a most valuable memoir, illustrated by figures of the highest exeellenee. Every natural group of organized beings, whether existing or extinet, would seem to have its epoeh of elueidation, a point of maximum in the history of its study, and the accumulation of faets towards that history. When the time eomes, the man is present for the work; but the right moment is ever preeeded by long series of preliminary labours, neeessarily more or less imperfeet, but not the less essential for the eventual right and full understanding of the subjeet. We are apt to forget when all is made elear to us, apparently as if in a moment, how we have been progressing step by step towards that hill-top from whenee we are enabled to eommand a full and fair view, and how every movement, though not always a straight one, summit-wards, was requisite for the attaiument of an eventual position, even though what we sought to see was hidden from us during our upward eourse. The so-ealled "diseoverer" is too apt to attribute to his own individual efforts what is really but the fruit of time, and the produee of the less fortunate labours of his predeeessors. This is not a fault of M. d'Arehiae; eonseientiously and earefully does he analyse and assign due eredit to the essays of those who have gone before him in the diffienlt and eurious study to whieh his monograph is devoted. Not fewer than 200 volumes, papers, or separate notes upon Nummulites (the work of 128 authors) are analysed in his treatise. First in the list is the aneient and venerable name of Strabo; among the latest are our eountrymen Carpenter, Carter, and Williamson, who have independently striven with remarkable ability and sueeess to elueidate the strueture of reeent and extinet Rhizopoda, attracted to the study by the same mysterious but fortunate impulse that has simultaneously direeted the attention of D'Arehiae, Rutimeyer, and numerous eontinental observers to the same interesting subjeet.

The author deseribes and figures 52 speeies of true Nummulites. Of these 20 are entirely new. But these numbers give no idea of the laborious task performed in sifting and reetifying synonymes, reeoneiling speeies in duplicate, and abolishing useless names. The

confusion that prevailed is instanced by the state of knowledge and nomenclature of some 22 species that were best known before. Of these 5 were placed in 2 genera, 3 in 5 genera, 2 in 4, 1 in 3, 1 in 6, 1 in 7, and 1 in 8 different genera. Among the species, 4 had received 2 names, 4 others 3 names, 1 four, 3 five, 2 six, 1 seven, 1 nine, 2 ten, and 1 eleven names; so that out of 22 true species of Nummulites no fewer than 98 reputed ones had been constructed!

M. d'Archiac divides the history of the study of Nummulites into five epochs. The first, or fabulous period, and by far the longest though of least importance, commenced with the writers of antiquity, and, after a long interruption during the middle ages, was resumed after the middle of the sixteenth century to extend into that of the eighteenth. The second period, more scientific than the first, but scarcely nearer the truth, extended from 1770 to 1804, when the sagacity of the illustrious Lamarck commenced to shed a new light on the affinities of the lower animals. The third period, from 1804 to 1825, was marked by numerous attempts towards a classification of the Rhizopoda, and Nummulites were described and figured with considerable care, the opinion of their Molluscan and Cephalopodous position prevailing in the writings of naturalists. The fourth epoch extended from 1825 to 1835, when zoologists seem to have settled into a fixed faith about the affinity of Nummulites and Foraminifera with Nautili and Cuttle-fishes (the doctrine taught two hundred and sixty years before by Conrad Gesner), and directed their attention closely to the structure and minuter classification of these curious bodies. It was marked by the commencement of the extensive labours of Alcide d'Orbigny among the Foraminifera. The fifth and final epoch commenced with the notable discovery by F. Dujardin of the low and Amœboid nature of the animal of the Rhizopod, and is signalised by numerous and excellent researches into the features and forms of existing and extinct Foraminifera. At length these living problems may be said to be understood, and the monograph by M. d'Archiac himself fitly closes their history for the present.

The high geological value to which the Nummulites and their order, the Rhizopoda, have speedily attained during the last fifteen years, contrasts curiously with the degradation they have as rapidly undergone during the same period in zoological position. Before 1835 they were generally regarded as Cephalopoda, and naturalists of repute were not wanting who went so far as to describe even the parts of the minute cephalopod that constructed the foraminiferous shell. That they were not Mollusca was scarcely suspected, though half a century before their lower nature had been, on slender grounds however, often maintained. The assumption of their elevated zoological position led to many an argument against support of the theory of the prevalence of a warm climate during the ante-tertiary epochs, from the fact of the abundance of chambered cephalopods in the ancient sea-beds of now cold or temperate latitudes. The abundance of minute chambered Cephalopoda in the North Atlantic at the present time, and their almost universal distribution, were confidently appealed to as conclusive against the inference. Their number in

the later formations, when the genera of Ammonitoida and Nautiloida had become scarce or disappeared for ever, was interpreted only as a continuance of the same class under new and minuter forms. Analogy was mistaken for affinity; and substitution of one group for one totally and organically different, although in the mere form of test not dissimilar, was mistaken for succession and representation within the sphere of one type. But the discovery of Dujardin led the way to an entirely new interpretation of the value of the Rhizopoda, and a new view of the part they play in time. Proving, from good evidence, to be among the lowest of animal forms, to be in fact Protozoa like *Amœba*, but differing from both Proteus and the animal element of the sponge by their investment with a hard and symmetrically arranged (generally in spiral symmetry) exo-skeleton, it is most interesting to note that their advent and maximum development have been, not during the apparent dawn of life, but amid the later epochs, and chiefly during those ages which many palæontologists regard as especially characterized by the highest forms of the animal kingdom. Indeed, so far as we know at present, the whole great group of *Protozoa*—the group that stands as it were at the very base and constitutes the rudiments of the animal series—is as characteristic of the tertiary section of time as the Vertebrata themselves are. A comparable phænomenon is becoming rapidly manifest in the mollusean subkingdom, now vastly increased by the accession of the Polyzoa to its ranks. These curious, lowly-organized, zoophytoid mollusca, instead of being the first of their type to appear, were preceded by members of all the higher orders of it, and do not become of much chronological value until the testaceous forms of the highest class of Mollusks occur, few and far between, and lose their strength and their importance.

The exquisite symmetry and regularity of conformation of the shells of most recent and fossil Rhizopoda were the chief sources of the errors that prevailed so long about their nature and zoological position. The true explanation of their structure appears to me to be that given in detail by our fellow-member Dr. Carpenter, to the effect that the entire mass, however symmetrical or regular, represents the products by successive gemmation originating from a single ovum. It matters little whether we regard each 'joint' or cell of a Nummulite as representing an individual or a zooid, provided we regard it as an element of the same essential nature with each polype of a polypidom, each cell-animal of a polyzoon, or individual of a Botryllus. The value of the regularity of the whole is not invalidated, because that whole is a compound and not a unity, and our faith in the specific value of the fossil, and its consequent geological importance, may be as strongly based on the constancy of characters whose diagnosis is drawn from the features presented by a congeries of individuals as from those presented by a single being. I make this remark, because the only serious objection that I can take to the views of M. d'Archiac touching the nature of the Nummulite concerns this fundamental point. When he states as an argument against its compound nature, that, if each of the cells were the proper

envelope of a particular individual, we ought to find a greater irregularity in their development in the same shell, and asks why, if this theory were true, should the heights of different coils of the same spiral present constant relations, and why the first and last cells should be less large than those of the median whorls, we cannot accept the objections, for a crowd of comparable phænomena presented by the Sertularian zoophytes, animals having considerable affinity with the Polyzoa, although of higher organization, come to our recollection. The variations of the Hydroida, their morphology and reproduction, bear too close a relation to the phænomena exhibited by the rhizopodous organism, to permit us to regard the Nummulite and its allies as simple bodies, or to dispute the theory of their gemmigerous constitution; in other words, the regulation of their organization by the law of paramorphosis.

The stratigraphical distribution of the Nummulites is especially of interest to the geologist. As compared with the grand scale of epochs, their reign was short, but it was well-marked and compact, and offers but one more proof to the thousands now known towards the demonstration of the unity of time-areas of natural genera, facts that should make us strongly hesitate before admitting the value of apparent and daily-decreasing exceptions, and that should give us fresh hope of the future attainment of a knowledge of the grand laws regulating life in its relations to time, and fresh faith in the biological section of the foundations of geology. The Nummulites characterize a portion, not the whole, of the tertiary epoch. Though once, and not many years ago, Nummulites were regarded to be as probably indicative of the cretaceous date of a formation as of its tertiary place, it would now appear that, between the nummulitic tertiaries and true cretaceous strata, deposits intervene, whose fauna and flora are such that we must regard them as of tertiary age. A most interesting and important feature of these deposits, traceable in the north-west of Europe, the south of France, in Savoy, in Switzerland, along the southern slopes of the Alps, in Istria, and even in India, is, that in numerous localities they exhibit evidences of a terrestrial origin, marked by the presence of coal, often accompanied by lacustrine shells, and sometimes by freshwater limestones. In facts of this kind we may get at the true explanation of the break between the cretaceous and tertiary faunas, without having recourse to prodigious cataclysms or paroxysmal elevations of mountain chains, which, if they did occur, as might have been the case, could have made far less impression on the distribution of animal and vegetable life, except in the immediate vicinity of the convulsion, than slow and almost imperceptible changes affecting gradually the disposition of the geography of a wide-spread area.

"The dial moves, and yet it is not seen," paradoxically writes an old poet. Time cannot progress without change, however slow may seem his course. The true measure of the extent and importance of a convulsion (as well as of the importance of unconformity), should be the amount of organic change that we can trace to a connexion

with the paroxysm. And yet what system of paroxysmal elevations has stood the trying test, when questioned on this principle?

It is of the Middle Eocene epoch—that section of the lower tertiaries of which the calcaire-grossier of the Paris basin may be cited as a central type and key-stone,—that the Nummulites are especially, and apparently exclusively, characteristic. The supposed carboniferous and oolitic Nummulites are of too doubtful a nature to be taken as exceptions. There is, it is true, a Nummulite (*N. intermedia*) found in the Miocene beds of Piedmont, and another (*N. garansensis*) in the Lower Miocenes of the Pyrenees. But I am not inclined to conclude with M. d'Archiac that these rare exceptions prove the existence of the last representatives of the genus after the Lower Tertiary fauna had disappeared, but rather to cite them in favour of the view that I have attempted to demonstrate, I trust successfully, when describing during the past year the Lower Tertiaries of the Hampshire basin,—to the effect that the so-called Lower Miocenes are essentially Lower Tertiaries and a portion of the true Eocene series, and that the passage from them into the Middle Eocenes is perfect and gradual, when we have for our examination an area presenting a full sequence of deposits.

Nevertheless it is not the less true that the nummulitic horizon is distinctly and definitely marked, and, from the frontiers of China and Thibet, even to the shores of the Atlantic, occupies a fixed position in the geological scale, a place above and succeeding the horizon of the lower tertiary lignites. The full demonstration of this great fact is a precious gain to our science; and when we consider what a vast area the nummulitic rocks occupy, what mighty mountains are made up of them, the prodigious accumulation of individuals of the fossils from which they receive their appellation, and the readiness with which their age can thereby be determined, we cannot but admit that the elucidation of their history has been a boon of no small value to comparative geology. This great tertiary formation extends across Europe, Asia, and Africa, forming a zone of 98° of longitude, comprised from south to north between the 16th and 55th degrees of latitude, and through much of its course exhibiting a breadth of 1800 miles. In the Himalaya, nummulitic rocks attain an elevation of more than 14,000 feet.

It will ever be a matter of just pride to our Society, that within our meeting-room and in our proceedings the main task was effected of clearing up the mist that clouded so long the geological history of the great nummulitic formation, and that here it was our indefatigable colleague, Sir Roderick Murchison, effected this great advance in tertiary geology. And now that the palæontology of the Nummulites has been made as clear as noon-day by the genius and labour of M. d'Archiac, it will ever be a matter of congratulation to us that the cabinets of our Society and the collections of its Members were freely and heartily placed at his disposal, and have proved of some value towards enabling him to perfect his researches.

The discovery, by Sir Charles Lyell and Mr. Dawson, of an am-

phibian related to new-world types, and of a probable land shell, of the family *Helicidæ*, in the interior of a fossil tree in the coal-measures of Nova Scotia, has excited general and deserved interest, and holds out a promise of future additions from unexpected sources to our roll of palæozoic animals. It is an event of no light significance. The number of palæozoic reptiles is steadily, though gradually, increasing at home and abroad. A new and highly curious form of Labyrinthodont from the Carluke Coal-shales, the *Parabatrachus Colei* of Owen, has appeared on this side of the Atlantic to support our hope of obtaining sooner or later a far larger list of palæozoic air-breathing animals than we at present possess.

Those who are interested in Permian palæontology will find a valuable contribution to this subject in the "Sitzungs-Berichte der Kaiserlichen Akademie der Wissenschaften" for June 1853. It is a memoir on the fauna of the German Zechstein formation by Baron Karl von Schauroth. A comparison is instituted between the German and English species (the latter as described by Professor King), and a concordance is given. From the lists in this paper it appears that there are 61 Permian species common to Germany and England; of these 21 are Lamellibranchiate and 17 Palliobranchiate bivalves. The total number of German species is 116, of which 21 are plants. In England we have 143, including 7 plants. The total number of known Permian species is stated as 237. In a contribution to the palæontology of the Triassic beds, an essay on the organic remains of the Muschelkalk near Jena, Dr. E. Smid enumerates 81 species. A striking feature of the assemblages of fossils in both Permian and Triassic series is the very small number of peculiar generic types. Of all the zones of life in time, these are the most unprolific in new and distinct generic types. Species they have in plenty of their own, but almost all belong to genera that are more important either above or below their horizon, than they are within them.

A long list might be given of recent papers on Oolitic, Cretaceous, and Tertiary palæontology, all more or less interesting, none without its value. For indications of most of these I would refer my hearers to the excellent 'Palæontographica' of Dunker and von Meyer, and the useful pages of the 'Jahrbuch' of Leonhard and Bronn. The memoirs by Reuss on cretaceous and other fossils are especially deserving of attention. In America, too, there is much doing in the study of organic remains. The vertebrata have found a most able investigator and describer in Dr. Joseph Leidy, who promises to be for the United States what Owen is to us. Much that I could wish to say on the progress of cretaceous and eocene palæontology I must for the present reserve; and of that of the newer tertiaries I will confine my remarks to an important work, yet uncompleted, the contents of which are equally worthy of notice at a time when the relations of the middle and lower tertiaries are subjects of discussion.

Most highly, indeed, to be commended is the admirably illustrated monograph of the miocene mollusca of the Vienna basin, published at the cost of the Austrian government and written by Dr. Hörnes, at whose disposal the fine collections of Partsch have been placed for

this special labour. The figures are exquisite. The fifth part appeared in 1853, and contained monographs of the species of *Ranella* and *Murex* (6 of the former and 43 of the latter genus). When this work is completed we shall be in a better condition than ever for deciding upon the *vexata quæstio* concerning the limits of the middle tertiaries. From the materials already before us we may obtain foreshadowings of the conclusion, and it may not be undesirable to offer a few remarks suggested by the facts recorded by Dr. Hörnes; in other words, by the fossil species he has so well described and carefully elucidated.

As far as the work has progressed, the genera monographed are canaliculated Gasteropoda; well-marked types, that are not likely to mislead, belonging to as many as 24 genera. Now, in looking over the lists of species in each, several points strike our attention, viz. 1st, the great development of species in certain tropical genera, or genera in the main tropical, such as Conus (19 species), Cypræa (10 species), Mitra (13 species), Terebra (8 species), Murex (43 species), Ranella (6 species); 2nd, the fact that the species of the more extensive genera are mainly extinct; 3rd, the fact that a considerable number of existing mollusks, characteristically Mediterranean, are present in this fauna; 4th, the presence of very few, scarcely any, existing forms not Mediterranean; 5th, the fact that whatever Celtic forms are present, such as *Cypræa europea*, *Erato lævis*, *Nassa incrassata*, *Chenopus pes-pelecani*, and *Murex erinaceus*, they are shells common to the Mediterranean and Celtic faunas, and therefore most probably original members of the former; 6th, the very large proportion of species common to tertiaries in the north of Italy and south of France; 7th, the small number of references to the Touraine Faluns, though those that occur are of considerable significance; 8th, the small number and doubtful character of the identifications with eocene species. This fauna seems as it were to have been the cradle of the existing Mediterranean fauna, but in the main to have been characteristic of the arms of a great previous Mediterranean, *whose main centre* was tropical, though not a portion of the Indo-Pacific provinces of our times. It seems to have had no northern communications, at least in the direction of Austria. Its tropical character is not derived at all from either the presence of eocene species or from the stamp of an eocene *facies*. Some great intervention of different physical conditions over a vast area must have separated its epoch from the latest eocene æra. It is decidedly not the fauna of the so-called lower miocene. A well-worked list of more than 150 species warrants the suggestion of these provisional considerations.

The search after and description of fossil plants has been actively prosecuted on the continent, and not a few memoirs, several of them beautifully illustrated, have appeared during the year. As contributions of facts towards a future understanding of Fossil Botany, these papers and figures are welcome and valuable; but as palæontological data for the service of the geologist, the use and appreciation of them requires the greatest judgement and caution. The vegetable unit in lists of extinct beings is of far inferior value to the animal unit, and

conclusions respecting the age and affinities of formations drawn from the fragments of an ancient flora should always be put forth as problematical and provisional. Yet in geological memoirs we too frequently find this caution lost sight of, or apparently unknown to their authors, who sum up the columns of animal and vegetable species alike, and add the numbers together, as if by diluting certainties with uncertainties we could come nearer a definite conclusion. Every botanist knows how difficult is the attempt to determine species of living plants from imperfect fragments, how slight is the clue in many cases afforded by a leaf, and how hopeless the task when he has before him only the fragment of a stem. Yet such are the materials from which in nine cases out of ten the describer of fossil plants constructs his species. Not content with indicating the possible or probable affinities of the morsel before him, he confers upon it the dignity of a generic and specific name, and enrols it in the catalogue of new types. When the specimen presents characters so positively different from any known form whatsoever, this proceeding may be excused; but such is not the excuse in the majority of instances. The nearer we approach our own epoch, the more difficult becomes the task, and the more are extreme care and forbearance demanded. With the greatest respect for the distinguished men who have of late contributed so much towards our acquaintance with the floras of the Tertiaries, I cannot but think that the positive nomenclature they have introduced into our lists is quite as likely to retard as to advance geology. Would that the warnings so often and admirably pronounced within our walls by my most able friends and fellow-members, Dr. Hooker and Mr. Charles Bunbury, were heard by some of the palæophytologists of Germany!

Among the most recent researches on this subject are the labours of Göppert on the flora preserved in amber. In this ancient resin portions of plants, even the flowers, are occasionally preserved as perfectly as the well-known insect remains that have so long excited the wonder of the curiosity-seeker, and yielded so rich a harvest to the entomologist. Of cellular plants 59 species were noticed thus embalmed by the eminent botanist just mentioned, and among them about two-fifths, and possibly more, as existing forms, Liverworts and Lichens being the prevailing identities. One Fern only is mentioned. The monocotyledons are restricted to the remains of an *Alisma*, a *Carex*, and portions of grasses. No fewer than 51 Gymnosperms are noted, and among them are identified *Thuya occidentalis*, an *Abies*, probably *canadensis*, and the *Librocedrus chilensis* of Chili! Of Angiospermous exogens 42 species were found. Among these are several regarded as identical with living types, as *Andromeda hypnoides* and *ericoides*, *Pyrola uniflora*, *Verbascum thapsiforme*, and *Sedum ternatum*. The whole list and the comments of the author are such as to excite the greatest curiosity, and to hold out hopes of fresh results from an investigation so likely to throw light on the climatal condition and geographical conformation of the northern hemisphere during the late tertiary epoch of the formation of the deposits in which the amber occurs. The same author has given an

account of the tertiary flora of Java, with a list of 38 species all marked as new. Dr. Ettingshausen has published a finely illustrated memoir on the Fossil Flora of the Monte Promina in Dalmatia, mainly of an eocene character. Out of 45 species enumerated, one is considered identical with a Sheppey species. Leaves referred to Proteaceæ and to tropical Leguminosæ and Laurineæ are among the more curious forms. But the objections I have made to the definitely naming of fragments must be held good against all these papers, and to the extensive and in many respects highly valuable memoir of M. Heer upon the tertiary flora of Switzerland.

Dr. Ettingshausen has made the tertiary flora of Häring in the Tyrol the subject of a finely illustrated and elaborate monograph, one of the many beautifully got-up scientific publications that have of late been issued at the cost of the Austrian government. He describes no fewer than 180 so-called 'species,' or, more properly speaking, portions apparently of different plants. Of these 73 are common to the floras of other localities; out of this number 41 are eocene, 9 miocene, and 23 species common to eocene and miocene. Protcaceæ, Myrtaceæ, and Leguminosæ form as much as a third part of this flora. The Flabellariæ and Chamæcyparites remind us of certain eocene plants of the Hampshire basin. Compared with existing floras the general aspect is Australian. The author infers that the climate was tropical, and ventures to pronounce on the probable mean annual temperature of the region in which these plants lived, determining it to be 18° to 21° Reaumur. In this conclusion, as well as in the decisions about species and genera, there is a degree of over-precision assumed to which fossil botany can justly lay no claim. In a previously published memoir on the tertiary flora of the Vienna basin, the age of the latter is stated to be miocene and the climate subtropical. In these determinations scarcely sufficient allowance is made for difference of locality and varying conditions, such as time of year of deposit and local elevation. The botanical differences between the plant-bearing beds of our own eocenes might lead to conflicting conclusions were we not well acquainted with their geological affinities.

M. de Zigno has announced the discovery of a new locality in the Vicentin for fossil fishes of the Monte Bolca type, and a rich tertiary flora probably of somewhat later age. Of greater consequence and general geological interest are his investigations in a stratum of grey Jurassic limestone containing vegetable remains at Monte Spitz de Botzo in the Sette commune of the Vicentin, first indicated by Fortis towards the close of the last century. The bed lies upon oolitic strata containing *Terebratula spheroidalis*, and is covered by others containing *Ammonites athleta* and *viator*, *Terebratula diphya*, and other organic remains indicative of the horizon of our Oxford clay and Kelloway's rock. M. de Zigno regards the plant-bearing bed as the equivalent of the Great Oolite, or thereabouts. He has obtained more than 400 specimens from the localities where it appears. All the plants are of terrestrial origin, and bear the strongest analogy to the oolitic floras of Scarborough and Mamers. The number of spe-

cies does not exceed 40, but the majority are new. They belong to the genera *Equisetites, Sagenopteris, Cycadites, Zamites, Otozamites, Araucarites,* and *Brachyphyllum*. The *Cycadeæ,* and especially the *Otozamites,* predominate. M. de Zigno is about to publish a monograph of his highly important discoveries, and it is to be hoped that British geologists will render him due assistance, the more so as all students of the English and Scottish oolites must feel greatly interested by this announcement.

But, on the risk of taking subjects out of the order of time, I must not omit to notice progress in the old and favourite direction of the vegetation of the carboniferous epoch and the origin and working of coal. The papers by Mr. Dawson and Mr. Poole on the phænomena of the coal-formation of Nova Scotia are contributions to this subject of very high interest, and are accessible in the pages of our own Journal. An excellent sketch, not without original matter, of the natural history of coal and the "Fossil Flora of the Mountain Limestone formation of the Eastern Borders," by Mr. George Tate, appended to Dr. Johnstou's delightful work on the Natural History and Antiquities of the Eastern Borders, well deserves the notice of the geologist and student of fossil plants. Circumstances of commercial interest have directed the attention of many men of science during the past year to the investigation of the nature of coal, and attempts at a strict and unmistakeable definition of what coal is has, I fear, after carefully reading all that has been said upon it, taken up in vain much of the time and thought of both philosophers and lawyers. Coal has become a geological chameleon. Opinions on this vexed question must necessarily vary according to the point of view, whether chemical, or geological, or mineralogical, or microscopical, at which we regard it. By making an *à priori* rule as to what coal should be, any man may arrive at a strict specific character, and more than one view of the matter may be right.

Petrological Inquiries.

The often-discussed subject of cleavage, about which so many geologists are at variance, has been treated in a fresh and novel manner by Mr. Sorby, who has communicated an essay of singular interest, "On the Origin of Slaty Cleavage," to the Edinburgh New Philosophical Journal for last year. This diligent observer has called the microscope to the aid of the hammer and clinometer. By an examination of extremely thin sections of rocks under high powers (that which he recommends as most generally useful for the purpose in view is about 400 linear), he has been enabled to throw new light on some of the greater geological problems; among others that of the cause of slaty cleavage. For the examination of slate rocks he recommends the use of a polarizing microscope. The physical structure and the optical properties of the component minerals may be identified thus, even when in grains less than $\frac{1}{1000}$th of an inch in diameter. A comparison of sections of uncleaved with those of cleaved rocks, having similar mineral composition, shows that the minute par-

ticles are differently arranged in each. The alteration of the arrangement in the latter case is such as would result from the rocks having suffered a change of dimensions, been greatly compressed in a line perpendicular to cleavage, and elongated to a certain extent in the line of its dip. Of these changes there are evidences afforded also by the diminution in the distance between any two points lying in the line of pressure in contorted beds, the dimensions of the beds in different parts of contortions, the change in the dimensions of the organic remains, and the arrangement of the green spots so generally seen in Welsh slates, and resulting probably from original concretions. These spots, Mr. Sorby remarks, in rocks without cleavage are almost perfect spheres, or are elongated in the plane of bedding. In cleaved rocks they are like the minute particles compressed in a line perpendicular to the cleavage, and more or less elongated in the line of its dip. The result of Mr. Sorby's inquiries is the strong support of the mechanical theory of cleavage, and a confirmation of the observations of Professor Phillips and, partially, of the views maintained by Mr. Sharpe, from whom Mr. Sorby differs in maintaining that the *particles* in general have suffered a change of position without actual compressing or crystalline arrangement. Mr. Sorby maintains that it is not possible to reconcile the mechanical facts noticed in his essay with the supposition of an electrical action or other non-mechanical agent being the efficient cause of the phænomenon of cleavage. By ingenious experiments he has been able to produce similar arrangements of minute particles with those observed by him in nature, all favourable to the theory which he so ably upholds.

In the West Riding Geological Proceedings, Mr. Sorby has a paper on the oscillation of the currents that drifted the sandstone beds of the south-east of Northumberland, and on their general direction in the coal-field in the neighbourhood of Edinburgh. By careful study of the minuter characters of the drift-structure in sandstone,—more minutely and closely than has hitherto been done,—Mr. Sorby proposes to arrive at definite results concerning the precise directions, characters, and velocity of the currents. The instances given in this essay, which may be regarded as the prodromus of more extensive memoirs, are most interesting, and warrant the conclusions at which he has arrived so far. I am convinced that the path chosen by Mr. Sorby is one of very great consequence to the future progress of geology, and that by methods similar to those which I have advanced and put in practice in the observation in the field of the distribution of organic remains in strata, viz. the observation and careful noting of phænomena, inch by inch, is as sure to yield valuable results to the purely physical as to the natural-history observer. The smallest of facts is not only worthy of notice and record, but may often prove to be the key by which we are enabled to acquire a philosophical knowledge of the rock-masses we are studying. The geology of no region, however extensive or however limited, can be said to be done until its minute as well as its more conspicuous constitution has been fully and fairly made out. Hitherto this has rarely been attempted, and perhaps our science is not yet ripe for an extensive employment of the method.

The microscopic researches of Mr. Sorby on the structure of fresh-water marls and limestones open out a new field for inquiry as yet little more than indicated. The idea of ascertaining the origin of the structure through a determination of the forms of the minute particles into which shells resolve themselves by decay, and of estimating the relative proportions of the microscopic ingredients of a rock by delineating on paper the outlines of the particles present in a thin section of the stone with the aid of the camera lucida, then cutting them out and weighing the figures of each kind separately, is a process I believe wholly new in geological research and due to our ingenious associate. The value of the proceeding may be tested by the results, which, so far as they are published, are excellent. So long as the microscope thus employed is guided by a practical geologist, our science will be a gainer by this kind of investigation.

The distinction of all granites into two species or varieties, each characterized by mineralogical and geological peculiarities, has been forcibly insisted on by M. Delesse, and illustrated from his researches among the rocks of the Vosges mountains. He distinguishes,—1st, the 'granites des Ballons,' containing little quartz, orthose in large crystals, felspar (of the 6th system), dark mica affected by acids, and frequently hornblende, ordinarily accompanied by sphene; and, 2nd, the 'granite des Vosges,' mainly made up of quartz and orthose, with the addition of a little felspar (of the 6th system), dark mica affected by acids, and transparent mica in smaller quantities not affected by acids. This granite often takes a gneissoid structure. The former kind is eruptive, and constitutes the more elevated portions of the granitic chain; the latter has rather the characters of a metamorphic rock. The distinction between the two sorts is not merely local, and has been observed by M. Delesse in not a few granitic localities; among other regions, in Ireland.

In a memoir on the mineralogical and chemical constitution of the rocks of the Vosges, M. Delesse discusses those phænomena of metamorphism characterized by felspathization, that is, by the development of crystals of felspar (of the sixth system) in ancient stratified rocks. To these felspathised rocks of the Vosges he applied the name Grauwacke, a term by which he proposes to designate every sedimentary rock, whatever be its age or structure, in which crystals of felspar of the sixth system have become developed. I am inclined to object to the revival of the name Grauwacke in the present stage of geological research; it has been used so variously, loosely, and indefinitely that it had better be wholly dropped from our nomenclature. The sense in which it is used by M. Delesse is not that in which the majority of geological writers have employed it, and since the class of rocks to which he would restrict the name are highly important and well deserving of specific distinction, the invention of a new term would not only have been excusable, but also of good service. The question may arise whether the apparent felspathization, in the sense in which this word is used by M. Delesse, may not in some instances rather depend upon the original diffusion

of felspar crystals through a sediment derived from showers of volcanic ash, constituting thus a rock of which numerous instances are familiar to the explorers of our palæozoic districts.

The line of research chosen by M. Delesse in the papers just noticed, and many others from his pen, is one sure to be productive of valuable results. The mineralogy of rock masses is of great consequence to the geologist, but to be satisfactorily treated must be dealt with by inquirers who are, like the author cited, practical geologists, and ready at the same time to avail themselves fully of the aid of chemistry. The treatment of the majority of simple minerals falls, in the main, within the sphere of the chemist; so much so, that we might almost be warranted in regarding mineralogy as the palæontology of chemistry.

Among the "general observations" prefixed to the new volume of M. D'Archiac's History are some brief but profound remarks on petrographical changes, and on the distinctions between the greater metamorphism of sedimentary formations and the lesser or metamorphism of contact, the latter being dependent on the action of igneous causes. The author calls attention to the fact of consolidation and tendency to metamorphism in the sedimentary strata of mountain masses, exhibited by the hardening of the limestones, their tendency to assume certain peculiarities of colour and frequently subcrystalline and even saccharoid textures, the conversion of the marls and sandy clays into schistose beds, and the indurated and compact characters of the sandy elements. On the other hand, the continuations of the same beds, when forming horizontal table-lands or extended plains, composed of conformable and undisturbed strata, exhibit entirely distinct mineral characters, being comparatively unsolidified and putting on very different features of colour and texture. These differences between the same set of rocks—in the one case disturbed, crumpled up and contorted, in the other resting almost in their original repose—are exhibited by formations of all ages indifferently, and would lead to the inference that the greater metamorphism is mainly due to energy of dynamical causes.

Although properly the subject should be mentioned under a distinct head, I may here allude to Mr. Tylor's interesting essay on the changes of sea-level effected by existing physical causes during stated periods of time, a paper abounding in suggestions of general interest and in curious calculations. As the author continues to pursue the same line of inquiry, it would be premature to discuss his conclusions now.

Still more distinct and far less practical in its theme is Mr. Saull's pamphlet treating of the connexion of geological phænomena with astronomical causes.

Text-books.

The spread of a love for geology among the people and students of science has its surest indication in the appreciation of text-books and synoptical treatises. The year 1853 has not been behind in affording evidences of the popular appreciation of our science. That great

standard of geological philosophy, the 'Principles of Geology,' by Sir Charles Lyell, has reached a ninth edition, one carefully and learnedly brought up to the ever-increasing knowledge of our day. A second edition has appeared of the 'Geological Observer,' a volume in which Sir Henry De la Beche embodies the fruit of years of fieldwork and reflection. A new elementary work of peculiar merit, entitled 'Popular Physical Geology,' has come from the pen of Mr. Jukes: this little book may be studied with advantage by the most experienced, and, keeping as it does, in a style highly commendable for perspicuity and nerve, the leading physical laws and facts of the science before the reader, unmixed with palæontological statements and conclusions, will serve as a wholesome corrective of a tendency to regard too exclusively its biological aspects, a bias on the part of geologists which a naturalist holds quite as much in dread as the sternest mineralogist or dynamical observer. A useful companion to elementary treatises in the form of an engraved table of the characteristic fossils of the several formations has been sent forth by Mr. Lowry. A new edition of Professor Pictet's 'Manual of Palæontology,' by far the best work of its kind, is a welcome contribution to our geological libraries; and the same may be said of Professor Phillips's 'Geology,' and of the lamented Dr. Mantell's 'Medals of Creation,' edited by Mr. Rupert Jones. Among elementary works that have appeared on the continent is one by the illustrious veteran Omalius D'Halloy, and in America Professor Hitchcock and Messrs. Adams and Gray have sent forth introductory treatises.

Conclusion.

In the course of this Address I have used some expressions that, as far as I am aware, are new to geological language, and involve an idea which, although hypothetical, I wish to put forth upon this occasion. I am strongly impressed with the belief, that, fanciful though it may seem, there is within it the germ of a great geological truth. I have spoken of genera concentrating towards the palæozoic pole, and *vice versâ*, of the substitution of groups, and the opposition of the more ancient to the mesozoic and modern faunas. The phrases have been incidental, and arose naturally out of the subjects under commentary, but the idea that lies at the base of them, whether true or fallacious, requires to be stated, and there cannot be a better opportunity than the present for venturing to start this fresh geological hare.

Every geologist whose studies have been equally or nearly equally directed to the organic phænomena of the three great sections of time usually received, Palæozoic, Mesozoic, and Tertiary or Cainozoic, cannot fail to have been struck with the greater value of the difference between the first or oldest section and the two newer divisions taken together, than between the first and middle terms and between the latter and the last. The degree of organic difference between the upper mesozoic and the lower tertiary epochs is rather more, but only slightly more, than the degree of difference between the lower

and upper sections of the great mesozoic period. But the gap between palæozoic and mesozoic, although the link be not altogether broken, is vastly greater than any other of the many gaps in the known series of formations. I am one of those who hold, *à priori*, that all gaps are local, and that there is a probability at some future time of our discovering gradually somewhere on the earth's crust evidences of the missing links. All our experience and knowledge, theoretical and practical, warrant the affirmation that at every known stage of geological time there were sea and land. Even those who believe in a primæval azoic period will hardly sanction the supposition that there has been any repetition of azoic epochs since the first life-bearing æra commenced. And if so, and if there were always sea and land since the commencement of the first fossiliferous formation, we are warranted in assuming that both earth and water had their floras and their faunas. All geological experience goes to show, that wherever you have a perfect sequence of formations accumulating in the same medium, air or water as the case may be, there is, if not a continuance of the same specific types, a graduated succession and interlacement of types and of the facies of life-assemblages: even as on the present surface of the earth the faunas and floras of proximate provinces intermingle more or less specifically, or, if physical barriers prevent the diffusion of species, assume more or less one general facies. This passage, by aspect and type, of one stage in time into another is but scantily indicated at present in the uppermost manifestations of the palæozoic life and the lowermost of the mesozoic. The missing links will sooner or later reward the diligence of the geological explorer.

But in the general aspect of the palæozoic world, contrasted with the worlds of life that followed, although all are evidently portions of one mighty organic whole, there seems to me to be something more than the contrast that depends on the loss or non-discovery of connecting links. There is more than we can explain by this theory. Granting for its support all facts capable of being so applied, there are residual phænomena to be accounted for, and which as yet have not been referred to any law that I know of.

For some years I have lived in hope of the discovery of a palæozoic fauna and flora more in accordance with those of after-epochs than those we know, and fondly fancied that local differences of physical conditions alone might account for the discordance. But the fields opened by Murchison, Sedgwick, and Phillips have been so extended and have yielded such rich harvests at the hands of James Hall and his fellow-explorers in America, and of Barrande, de Koninck, de Verneuil, the Römers and Sandbergers, M'Coy, King, Salter, Roualt, and many other able palæontologists who have worked at palæozoic fossils in Europe, that it is becoming evident that we have before us a fair and true image of at least the marine aspect of the primæval group of faunas. The more they are investigated, the wider the ground is explored, the more striking is the difference in the main between the life palæozoic and the after-life.

Doubtless a principal element of this difference lies in *substitution*—in the replacement of one group by another, serving the same pur-

pose in the world's œconomy. Paradoxical must be the mind of the man, a mind without eyes, who in the present state of research would deny the limitation of natural groups to greater or less, but in the main continuous, areas or sections of geological time. Now, that greater and lesser groups—genera, subgenera, families, and orders, as the case may be—or, in truer words, genera of different grades of extent—have replaced others of similar value and served the same purpose or played the same part, is so evident to every naturalist acquainted with the geological distribution of animals and plants, that to quote instances would be waste of words. This replacement is *substitution of group for group*—a phænomenon strikingly conspicuous on a grand scale when we contrast the palæozoic with the after-faunas and floras. A single instance of these greater substitutions may be cited to assist my argument, viz. the substitution of the Lamellibranchiata of later epochs by the Palliobranchiata during the earlier. In this, as in numerous other instances, it is not a total replacement of one group by another that occurred; both groups were represented at all times, but as the one group approached a minimum in the development of specific and generic types, the other approached a maximum, and *vice versâ*. I think few geologists and naturalists who have studied both the palæozoic and the after—I must coin a word —*neozoic* mollusca will doubt that a large portion of the earlier Brachiopoda—the Productidæ for example—performed the offices and occupied the places of the shallower-water ordinary bivalves of succeeding epochs.

Now in this substitution the replacement is not necessarily that of a lower group in the scale of organization by a higher. There is an appearance of such a law in many instances that has led over and over again to erroneous doctrines about progression and development. The contrary may be the case. Now that we have learned the true affinities that exist between the Bryozoa and the Brachiopoda, we can see in these instances the *zoological* replacement of a higher by a lower group, whilst in the former view, equally true, of the replacement of the Brachiopoda by the Lamellibranchiata, a higher group is substituted for a lower one. Numerous cases might be cited of both categories.

But can we not find something more in these replacements and interchanges than mere *substitution*, which is a phænomenon manifested among minor and major groups within every extended epoch? Is there no law to be discovered in the grand general grouping of the substitutions that characterize the palæozoic epoch when contrasted with all after-epochs considered as one, the Neozoic? It seems to me that there is, and that the relation between them is one of contrast and opposition—in natural-history language, is the relation of Polarity.

The manifestation of this relation in organized nature is by contrasting developments in opposite directions. The well-known and often-cited instance of the opposition progress of the vegetable and animal series, each starting from the same point—the point at which the animal and vegetable organisms are scarcely if at all distinguish-

able,—may serve to illustrate the idea, and make it plain to those to whom the use of the term Polarity in geological science may not be familiar. In that case we speak of two groups being in the relation of polarity to each other when the rudimentary forms of each are proximate and their completer manifestations far apart. This relation is not to be confounded with divergence, nor with antagonism.

If we take the scale of geological formations, representing the succession of the leading divisions of time, and note for each of the epochs the known generic types present during its duration, we shall find there is not an equality of production, so to speak, at all times of fresh generic ideas. Genera have appeared, as it were, in batches. I am forced to use expressions that seem almost irreverent, and a phraseology of a loose and popular kind, in order to convey the more vividly my meaning. To talk of the appearance of a genus, that is, the appearance of an ideal type, is loose language I am aware, but its meaning or intention can scarcely be misunderstood. In the individuals of a species only can we have the embodiment of a generic idea; but in discussing a question of the kind I am considering it is convenient to use the word genus as if it were a realized unit and an entity. We speak, as it were, through a diagram. Now if commencing, upon our scale, at the dawn of the palæozoic epoch, and noting the beginning of genera or groups from the first known fauna up to the advent of man at the termination of the so-called tertiary epoch, we cannot fail to perceive the following general facts:—

1. During the earlier and middle stages of the palæozoic epoch there was a great development of generic ideas.

2. During the middle and later stages of the neozoic epoch there was a great development of generic ideas.

3. During the terminating stages of the palæozoic epoch the origination of generic ideas was very scantily manifested.

4. During the commencing stages of the neozoic epoch the origination of generic ideas was very scantily manifested.

5. The majority of generic ideas that originated during the palæozoic epoch belong to groups (of various degrees of generic intensity) which are characteristically palæozoic, *i. e.* have their maximum development and variety during the palæozoic epoch, or else are even exclusively palæozoic.

6. The majority of generic ideas that originated during the neozoic epoch belong to groups which are characteristically neozoic in the same manner.

7. The minimum development of generic ideas in time is at or about the passage or point of junction of the palæozoic and neozoic epochs.

8. Groups characteristically palæozoic swell out, as it were, in a direction *towards*, not from, the commencement of the palæozoic epoch.

9. Groups characteristically neozoic swell out in a direction *from* the commencement of the neozoic epoch.

That there are apparent exceptions to these general facts I do not pretend to deny, but the rules are so much more powerful than the

exceptions that we may safely wait with confidence for the explanation of the seeming anomalies during the course of the progress of research.

Now there is but one conclusion that can be drawn from these facts, if after being tested with every evidence now known to us they remain intact as our science progresses. This conclusion is to the effect, that the relation betweeu the palæozoic and neozoic life-assemblages is one of development in opposite directions, in other words, of *Polarity*. Iu the demonstration of this relation it seems to me that we shall, in all probability, discover the secret of the difference between the life anterior to the Trias and the life afterwards. The notion is in some degree a metaphysical one, but not the less capable of support through induction from the facts. I plead for its consideration, believing it to be worthy of earnest inquiry. I know that its novelty and seeming vagueness may repel many when it is thus briefly, and as if in outline, put forth. But before any geologist or naturalist rejects it, I would ask him to study carefully the admirable monographs, written without a bias, of whose merits I have been discoursing in this Address; to seek out the manifestation of the idea in the first instance in some important and characteristic group of beings about whose time-distribution we have now a sufficient knowledge, such an assemblage as the Trilobites described to us in the work of Barrande, or the Brachiopoda as exhibited in the monograph by Davidsou; to take and analyse the ample lists of extinct beings marshalled in the pages of Morris, or in the more general muster-rolls of Broun and Alcide d'Orbigny; and then, having done this, to consider earnestly and fairly the idea that I have ventured to suggest of *the manifestation of Polarity in Time*.

Gentlemen, since I have occupied this Chair I have heard two reproaches cast upon our Society, the one that we throw cold water upon theories, and the other that we are opposed to the practical applications of Geology. The fate of the concluding paragraphs of this Address will not, I hope, be confirmatory of this first accusation, one seldom urged against Geologists. As to the second, I believe I speak the sentiments of every working geologist in this Society, when I say that uo papers, no discussions within these walls, are heard with more pleasure or received with more approbation than those which have a practical and economic bearing, always providing that sound science and original research constitute their foundation. Empiricism we eschew and abhor. Solid knowledge, careful observations, and sound scientific theory are as necessary for economic as for unremunerative geology. During the Session just concluded the various aspects of our science have each had an impartial share of attention. In the Session which is about to commence we have every prospect of holding our forward course in the sound and safe path that the Geological Society of London has chosen from its beginning.